# PAPERS RELATING TO POLITICAL ECONOMY

BY

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VOLUME I

*Published on behalf of the Royal Economic Society by*

MACMILLAN AND CO., LIMITED
ST. MARTIN'S STREET, LONDON

1925

PRINTED IN GREAT BRITAIN

# CONTENTS

## Section I.—VALUE-AND-DISTRIBUTION



## Section II.—MONOPOLY



## Section III.—MONEY

# INTRODUCTION

These volumes contain articles and reviews which appeared in the Economic Journal during the first thirty years of its existence (1891–1921 inclusive). The republication is undertaken by the Royal Economic Society acting through its Council. I highly appreciate the honour of appearing for the second time under the auspices of the Society. Where that honour could not be regarded even by the partiality of an author as deserved, I have not availed myself of the permission to reappear.

It may be proper to state more fully why on behalf of certain writings I have not accepted the handsome offer of the Society. The omissions fall under four heads. There are, firstly, passages which involve erroneous reasoning. I have noticed only two or three passages which deserve to be placed in this category. But I dare say that kind critics will add to the number. Next are controversial writings which may be described as "intricate"; in that they refer to other writings not quoted in full and probably not present to the reader's mind. Controversial matter in which are mixed up what the author said and what the critic said is apt to distract and offend the reader; especially after the lapse of years, when interest in the subject has died down. The objection was not equally applicable to the original publication of the said passages at a time when the questions were burning and the arguments disputed were easily recognised—often contained in then recent numbers of the Economic Journal. A further objection to reproducing portions of bygone controversies is that injustice may be done to a writer by quoting separately some particular utterance apart from the general tenor of the author's thought. This motive is not entirely altruistic. For I am sensible that the reproduction of some of my contentions, as they stand in the Economic Journal, might produce an unfairly unfavourable impression. In particular, the arguments which I have employed against distinguished writers in defending the thesis of Torrens and Sidgwick, that the introduction of Free Trade might possibly prove injurious to a nation, do not stand well alone; they require to be qualified

by the explicit admission made subsequently, that in fact the supposed case, though possible, is rendered improbable by the probability of finding employment for labour in general, in the long run—what Professor Pigou, referring with approval to this explanation, describes as the Elasticity of the Demand for Labour. These considerations have called for several omissions. Yet the reader need not be afraid that the spice of controversy will be wanting to the Collection. A third class of passages are omitted on the ground of what may be called excessive elaboration. It is not intended thereby to attribute excess to the original publication. What is worth saying once may not be worth repeating. For instance, I have not thought it useful to reproduce the long note occupying four pages of small print in the ECONOMIC JOURNAL for 1910 (p. 300): "on the probability of a tax on one of two articles which are partially substitutes for each other producing a fall in the prices of both articles; in a regime of monopoly." And yet it may have been worth while once for all to array the received principles of Probabilities against the authority of a distinguished economist who had derided the possibility of the two articles becoming cheaper in consequence of the tax. In a fourth category I place reviews, which are merely declaratory of a book's contents, and perhaps of the critic's summary opinion as to the worth of the book. In the same limbo may be placed some biographies, and numerous abstracts of official reports and other publications; mostly unsigned, some initialed. The second and third grounds for rejection, but not as far as I can judge the first, have sometimes conduced to the exclusion of a review. Wherever a passage of any significance in the original has been omitted, the reader's attention has been called to the omission. But I have not thought it necessary to notice every abridgment of a paragraph or alteration of a phrase that I have taken the opportunity of introducing.

The Royal Economic Society have placed me under an additional obligation by including in this Collection papers dealing with economic subjects which I have contributed to the organs of other learned Societies. The inclusion of this set extends the period which the Collection covers by three or four years—back to the later 'eighties of last century.

A collection of papers written at different times and for various destinations is naturally deficient in unity of design. I have endeavoured to palliate this defect by re-arranging some of the papers under five comprehensive headings, namely, Value-and-Distribution, Monopoly, Money, International Trade, and

Taxation. There remain over writings which fall into two classes constituted by cross-divisions, namely Mathematical-Economics and Reviews.

This classification is not perfect: the place of each piece could not always be predicted from the definition of the section in which it occurs. It may be useful, therefore, to prefix to each of the collected papers a short description of its purport. In making these prefatory explanations, I have resisted the temptation to prolixity which the opportunity of being my own interpreter presented. I hope to escape the fate of that pretentious host who, as described by the satirist, so bored his guests by descanting on the qualities of the viands, that they revenged themselves by not tasting any of the good things—

> "Suaves res, si non causas narraret earum et
> Naturas dominus."

Additional clues or links are afforded by some new footnotes, indicated by asterisks. There are also appended to some of the papers new notes, enclosed in square brackets. Additions to existing notes are enclosed in square brackets; all except references to pages in the present volumes. I have added also, for the further convenience of the reader, an Index referring to topics on which I have endeavoured to shed light. Sometimes a topic is best introduced by the name of a writer who has made it his own. But the only names mentioned in the Index are those of writers whom I have criticised, or at least characterised, whether favourably or otherwise. The Index would have to be much enlarged if it was to include the names of all those to whom in the text I have acknowledged indebtedness. Among them would be many of those who have now collectively placed me under a new obligation, the Members of the Royal Economic Society.

F. Y. Edgeworth

*All Souls College,*
*Oxford,*
1924.

# SECTION I

## VALUE-AND-DISTRIBUTION

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## VALUE-AND-DISTRIBUTION

### (A)

### THE OBJECTS AND METHODS OF POLITICAL ECONOMY

[This is an inaugural lecture delivered in 1891 on the occasion of entering on the duties of Professor of Political Economy at the University of Oxford. The address naturally contains much that is special to the place and the occasion; but there may be some reflections of more general interest.]

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Many of those who have spoken on occasions similar to the present, have signalised their entrance on the work of a Professor by indicating the scope and method of the science professed. It was thus that my illustrious predecessor, Senior, in the introductory lecture on Political Economy which he delivered before the University of Oxford almost two-thirds of a century ago, described the provinces of theory and practice, and the wide and slippery interval by which they are separated. So Dieterici—a great name in the annals of statistics—in his inaugural address to the University of Berlin,[1] almost as long ago, showed the opposite errors of " mere philosophy and mere experience." In fine, not to multiply authoritative instances, the present occupant of the chair of Political Economy at Cambridge,* on his accession to that eminence, gave a memorable discourse on the present position of Economics. I follow these precedents in the choice of a subject; I cannot follow them in the originality of its treatment. *Difficile est proprie communia dicere;* I shall endeavour to appropriate to the present occasion reflections which others have made common property.

In this spirit, approaching first that part of our subject which authorities on method distinguish as abstract or theoretical, I

[1] *De viâ et ratione economiam politicam docendi,* Berlin, 1835.

* Alfred Marshall.

submit that there is a certain congruity between the theory of political economy and the studies which are particularly characteristic of this university, the great Oxford school of *literæ humaniores*. For the ideal of demonstrative science which is obtained from the study in that school of the ancient philosophy and modern logic appears to be fulfilled in political economy alone, or chiefly among the studies of which man is the direct object. It is in economics only, when we have excepted the mathematical physics, that there is realised with some perfection that type of science to which Greek thought aspired, which Aristotle taught if he did not practise: the leading up to general principles and leading down to particular conclusions. The logical methods, which are studied in the School of *literæ humaniores* may be exemplified in political economy without going beyond the range of subjects conterminous to that school.

The demonstrative part of political economy, to which I am referring, seems rudimentary, when compared with mathematical physics. But though our trains of reasoning are short, they are not simple. Consider any of the problems which Ricardo delighted to put. A tax is imposed on manufactured commodities and the proceeds expended in a bounty on agricultural produce (or vice versa); how will different classes be affected? Or, take a question in which a characteristic difficulty of our science —the disturbing influence of interest and passion—becomes felt. What would be the effect of limiting the hours of labour upon any definite supposition as to the numbers and efficiency of the previously unemployed class? Such questions are much more difficult than they seem. It is here, as has been observed of the calculus of probabilities: the first appearances are generally fallacious. But, whereas that calculus is handled only by experts, we all, learned and unlearned, theorise about political economy. Abstract reasoning, far from having become obsolete, seems never to have swayed larger masses. How many hundreds of thousands of Continental Socialists have been bred on the Hegelian subtleties of Marx! It cannot be supposed that such mystic formulæ are altogether of the nature of incantations, sung by those who are preparing to use the knife. Reasons honestly urged can only be met by reason. The statesmen of the coming generation must be prepared to separate what is true from what may be misleading in answers, such as the following, which are given in influential quarters to one of the questions which I have proposed. To reduce the working hours, it is said, would materially increase wages, by providing work for many who are

now in enforced idleness; because new demands would be made for commodities, resulting in a large increase in production and cheapening of commodities. What is the portion of truth in the common belief that a reduction of the hours of work would raise wages generally merely by causing an increased demand for labour, and independently of more indirect effects?

It may be observed that correct theory on such subjects has a use beyond its immediate application to practice, a dialectic or controversial use. Those who appeal to theory shall go to that tribunal, even though it is not final. There is here a legitimate sort of *argumentum ad hominem;* for which it is not very easy to find a parallel among the older sciences. The state of speculation which still prevails with respect to industry might be illustrated by the science of war upon the following fanciful supposition. Suppose that the authorities of the War Office—or those aspiring to office—were to recommend rules of gunnery, formulæ for the flight of projectiles, based upon a theory of gravitation other than the Newtonian. The simplest method of meeting these proposals —and estimating the authority of those who made them—would be to present the true theory of motion *in vacuo;* though, of course, that theory requires to be modified by complicated corrections for the resistance of the air, before it will enable us to hit the mark in practice.

The grotesqueness of my illustration brings into view a peculiarity of our study: that in the race of the sciences we are as it were handicapped by having to start at a considerable distance behind the position of mere nescience. An effort is required to remove prejudices worse than ignorance; a great part of the career of our science has consisted in surmounting preliminary fallacies.

Now in overcoming these initial obstructions academic training is likely to be of great use. Philosophic culture is calculated to eradicate the weeds of fallacy which grow nowhere so rank as in our field. Indeed many of the difficulties which beset political economy are common to morals and metaphysics. There is a similar inability on the part of those who have been bred in different speculative systems to enter into each other's positions; there is the same vulgar contempt for all speculative systems in uncultivated minds. There is a similar plurality of plausible hypotheses—a sort of kaleidoscopic change of views, with the turn of the fashion in speculation. For example, just as in morals the theory which resolves virtue into self-interest really accounts for a great part of the phenomena and, leading to by

no means the worst sort of conduct, as Bishop Butler shows, has sometimes caused oblivion of an older and a higher theory; so in political economy the theory which explains value by utility—utility in the sense defined by Jevons—has so fascinated by no means the worst sort of economists, that they have almost forgotten, or at least degraded, the older, and in some respects more important theory which connects value with sacrifice and labour. There is ever a danger that, as we press on to seize new conceptions, we should lose the positions which have been already won. Hence the history of theory is particularly instructive in political economy as in philosophy. History and literature, dialectics, and all that the Greeks comprehensively called *words*, seem the best corrective of the narrow prejudices and deceptive associations which are sure to be contracted by those who have been confined to a single school or system. Words indeed in a literal sense require the attention of the economist as well as the philosopher. For there is in both spheres a danger of double-meaning terms; a demand for discriminating definition. In fact it has been seriously proposed by one of our greatest thinkers both in philosophy and political economy to revive the Platonic search for definition as a method of economic investigation. So cognate are the studies of political economy and *literæ humaniores*.

It must not however be understood that economics are altogether of the complexion of literature and the humanities. There is a certain affinity between the mathematical physics and the one social science which is largely occupied with measurable quantities. The nature of things which has involved the knowledge of physics in the mysteries of mathematics has not wished the way of cultivating economics to be altogether free from that difficulty.[1] In the memorable words of Malthus, "Many of the questions both in morals and politics seem to be of the nature of the problems *de maximis et minimis* in fluxions."[2] The differential calculus, the master-key of the physical sciences, unlocks the treasure-chamber of the pure theory of economics. I do not deny that the refinements of pure theory may be reached by the use of ordinary language, sufficient circumlocution being employed; the treasure-chamber has a key of its own, but it is a cumbrous one. Nor do I attribute to the mathematical picklock the intricacy of the wards which guard the more recondite treasures of the higher physics. On the contrary, there is

[1] "Deus ipse colendi Haud facilem esse viam coluit."—VIRGIL, *Georgic* i.

[2] Bonar's *Malthus*, p. 225.

required but little strengthening and filing of the instruments which are in common use. The well-known economists who say that the cost of labour is a function of three variables and that demand and supply always tend to equilibrium[1] use terms which are but paraphrases of the mathematical language which is the mother-tongue of the calculus of *maxima and minima.* The advantage of employing that language might perhaps be compared to the advantage of studying the ancient philosophers in their native tongue. I do not mean that the mathematical method should form part of the curriculum, as we make Greek obligatory for the students of philosophy. But may we not hope that the higher path will sometimes be pursued by those candidates who offer *special subjects* for examination?

Still referring to the theoretical part of political economy, I come to the question: What is the use of abstract theory: the positive, as distinguished from the controversial use which I have indicated as extensive and important? I hope that in academic circles it may be allowable not to construe use narrowly. There still is room for the studies which the Greeks attributed to theoretical science, as distinguished from practical sagacity, which Aristotle[2] characterised as wonderful, and hard to be attained to, and sublime, but not immediately useful, not directly applied to the service of humanity. Of this character are the higher generalisations of economics, whether expressed in words or symbols, in the language of Ricardo or of Jevons. Such is the theory of the dependence of value upon cost, of the adjustment of remuneration to efforts and sacrifices; like the surface of the sea—a sluggish sea with viscous wave—slowly settling to equilibrium. Such is the theory of the extension of demand in the different directions of consumption to one and the same limit of satisfaction; like an imprisoned gas pressing equally at all points against its boundary. Such is the theory, less familiar and less easily imaged, which is formed by combining the conceptions of Jevons and of Ricardo,[3] and deducing the whole system of values and remunerations from the single simple principle that each individual seeks (subject to given conditions) simultaneously to maximise the pleasures of consumption and minimise the unpleasantness of production.

From these heights of speculation, as from a lofty mountain, may be obtained general views as to the directions in which

[1] *Cf.* Mill, *Principles*, Book II. ch. xv. p. 7; Book III. ch. iii. p. 2.

[2] *Ethics*, Book VI. chap. viii.

[3] *Cf.* Sidgwick, *Principles of Political Economy*, Book II. ch. ii.; Marshall, *Principles of Economics*, 2nd ed. pp. 544, *et passim.*

practice trends. Such a general direction has been afforded by the Ricardian theory of the rent of land. Such a general direction will probably be afforded by the theory of consumers' rent which is connected with the names of Marshall and Dupuit: from the view that members of a community have an interest in each others' expenditure; that regulations encouraging the consumption of much-manufactured commodities rather than rawer material are *primâ facie* expedient; and that the success of a government work as a business undertaking is not the pecuniary measure of its advantage to the community.

But while we indulge these general views, we must ever remember that they are but distant. It is only at the heights that contemplation "reigns and revels." The descent to particulars is broken and treacherous; requiring caution, patience, attention to each step. Those who without regarding what is immediately before them have looked away to general views, have slipped.

It is worth while to consider why the path of applied economics is so slippery; and how it is possible to combine an enthusiastic admiration of theory with the coldest hesitation in practice. The explanation may be partly given in the words of a distinguished logician who has well and quaintly said that, if a malign spirit sought to annihilate the whole fabric of useful knowledge with the least effort and change, it would by no means be necessary that he should abrogate the laws of nature. The links of the chain of causation need not be corroded. Like effects shall still follow like causes; only like causes shall no longer occur in collocation. Every case is to be singular; every species, like the fabled Phœnix, to be unique. Now most of our practical problems have this character of singularity; every burning question is a Phœnix in the sense of being *sui generis*. We have laws almost as simple and majestic as that of gravitation, in particular those relating to value and distribution; but these laws do not afford middle axioms, such as the proposition that planets move in ellipses deduced from the law of gravitation. So dense is the resisting medium which obstructs the free movement of the market; and not only in general dense, but also variable from case to case. The impediments to free competition are different in the cases of the money market and the labour market; and not very easy in any case to be accurately estimated so as to allow of scientific prediction. Repeated experiments in exactly similar conditions, such as those by which a physicist obtains empirical laws for the resistance of the

atmosphere to projectiles, are not available in the practice of political economy.

Often indeed the resisting medium is invisible as air, and its presence escapes attention. There occurs the difficulty of perceiving all the data which should be taken into account in our reasoning. For, as it has been said in a well-known essay by John Stuart Mill—one of those who have rendered it superfluous at the present day to discourse at length upon the method of political economy—" Against the danger of *overlooking* something, neither strength of understanding nor intellectual cultivation can be more than a very imperfect protection."

As an instance in which eminent theorists may have omitted a relevant circumstance may be taken the question whether it is possible for trade unionists by standing out for a higher than the market rate of wages to benefit themselves permanently without injuring other workmen. The negative answer which has sometimes been given omits the consideration that an increase of wages tends to increased efficiency, and increased efficiency to increase of the produce to be distributed among all the parties. There are those who attach much weight to this consideration. How much weight should be assigned to it is a question of a sort which often baffles the theorist: to determine the quantity, after you have assigned the quality or direction of an agency. The possibility that diminished hours of work will not cause a proportional diminution of work done may be instanced both as a material consideration which has often been left out of account by serious reasoners in old times; and one of which it is not easy to determine the force, as well as the direction.

Against " the danger of overlooking something," no remedy can be prescribed except to cultivate open-mindedness and candour, and above all sympathy, the absence of which has aggravated the most serious mistakes which have been committed in political economy. I refer particularly to errors relating to the remuneration of the wage-earning classes. Slips accidentally committed by the great theorists through carelessness or the passion for simplicity would probably have been far less serious, if those who interpreted political economy in the press and in parliament and applied it in the conduct of their business had entered more fully into the life and conditions, views and wants of the wage-earners. A generous caution would have softened the harsh tenets that the introduction of machinery could not ever be detrimental to workmen, that the Factory Acts were a mischievous interference with the liberty of the labour-market, that workmen could

not possibly benefit themselves by union. I would dwell longer on this all-important topic—the conduciveness of good-feeling to wisdom—if I were able to convey a feeling by a discourse. I can best express myself by pointing to an example which will be present to the memory of all here, the example of ardent sympathy perfecting reason which is afforded by the noble life of Toynbee.

To return to what I was saying about the difficulty—even when you have perceived a relevant consideration—of rightly appreciating its weight, there is a specific for this failing, namely statistics. Statistics are an indispensable part of the equipment of the modern publicist; and it is truer now than in Plato's time that he who has no regard for the art of counting will not be himself of much account. It will be my duty to take occasional opportunities of discoursing on the methods of statistics—the logic of numbers, in which fallacies unfortunately form a large chapter.

When we have done our best to correct our practical judgments, there will still be, as Mill says, "almost always room for a modest doubt as to our practical conclusions." This modesty and this doubt are particularly appropriate in the case of the academic teacher, who, expected to know something about all the branches of his subject, cannot be expected to have examined many of them closely and at first hand. In the balance of judgment he may measure those weights which, so to speak, are most regularly shaped and admit of theoretical determination; but he must be ever prepared for the balance being turned by practical considerations of which he has not taken due account. Therefore he should "teach, not preach," in the words of Professor Walker. Or, as it has been said by another eminent American economist, Professor Dunbar, a high authority on method (in a recent essay on the "Academic Study of Political Economy") [1] the instructor is not concerned with "the propagation of his own views. He is interested in making his reasoning process clearly understood; but this is because of the value of the logical process itself." Professor Dunbar specifies several good reasons why "the teacher's opinion upon some burning question of the day" should not be communicated to his pupils. There occurs to me as pertinent another case in which the teacher will not give an opinion—he may not have got one.

Having dilated at such length on theory and its application to

[1] *Quarterly Journal of Economics*, July, 1891.

practice, I am unable to devote proportionate attention to the advantages of historical studies. But you will not expect me to expatiate upon advantages which are known to most of you from personal experience. I will only advert to a secondary and less obvious benefit attending historical researches. To trace the affiliation of ideas in the progress of science is calculated to correct our estimates of authority: to reduce in general the extravagant regard which the youthful student is apt to entertain for contemporary leaders, and to assign due weight to real originality.

It is impossible to overrate the importance of the historical method; understanding it in the sense defined by one of those who have most ably recommended and practised it, Professor Ashley,[1] as "direct observation and generalisation from facts past or present." I do not pretend to determine with precision the parts played by theory and history in this sense; I would as soon attempt to solve the old dispute, whether nature or man does more in the production of wealth. As the producer of wealth will push his investment in the different agents of production up to a certain limit which has been called the "margin of profitableness"; so, in the manufacture of economic wisdom, each of us should expend his little fund of energy, partly on the fixed capital of the deductive *organon* and partly on the materials of historical experience. The margin of profitableness in the intellectual as in the external world will differ with the personality of individuals. No general rule is available, except that, like the cultivated Athenians,[2] we should eschew the invidious disparagement of each others' pursuits. I rejoice that such illiberal jealousy among the votaries of economic science is becoming as obsolete as the Battle of the Books. As it has been well said by one among us, Mr. Price, "The quarrel between the 'old' and 'new' economists seems to be giving way on all sides to a hearty desire to recognise good work wherever it is to be found, and to an honest endeavour to seek for grounds of agreement rather than reasons for difference." [3]

In this broad and liberal spirit our school of modern history has included political economy among its studies. In this spirit the teachers of both subjects will, I hope, cordially co-operate.

While referring to the historical side of political economy, I cannot but think of my immediate predecessor,* whose brilliant

[1] Inaugural Address, University of Toronto.
[2] Pericles apud Thucydiden.
[3] Economic Journal, No. 3, p. 509.
* Thorold Rogers.

achievements have reflected lustre on this University; who not only extracted the crude ore of historical material from the dim and dusty mine of mediæval records, but also himself elaborated, purified, polished the precious mass for permanent use and solid ornament. Nor can I be unmindful of the first occupant of this chair, of Senior, who, while advancing the boundaries of the science at almost all its frontiers, was at the same time versed and active in affairs, and contributed to history by recording the opinions of the men who made history.

When I remember the distinguished publicists who have occupied this chair, I am conscious of the deficiencies of their successor. I can but promise that zeal in academic teaching will not be deficient. I venture also to indicate a more external advantage which is likely to conduce to the usefulness of my office. I allude to the opportunity of collecting contemporary opinions and events—as it were into a focus—which is afforded by the position of the editor of the journal which is the organ of the British Economic Association.* In furthering the objects of that Association I hope for much assistance from my fellow-students in this University.

* Now the Royal Economic Society.

## (B)

## THE THEORY OF DISTRIBUTION

[THE Theory of Distribution is the substance of lectures delivered at Harvard University in the autumn of 1902; and published, in the form now reproduced, in the *Quarterly Journal of Economics*, February, 1904. The Theory of Distribution is treated as involving the general theory of Value determined by Supply and Demand in so far as in a regime of competition the shares of the parties are settled by that process. The general principle is contemplated in its application to the three markets in which the entrepreneur deals with the respective owners of the classical factors of production. Here, as more fully in papers included in the mathematical section, there is disputed the dogma that the remuneration of the entrepreneur is null; together with the less paradoxical tenet that his gain is exactly equateable to the loss which the community would suffer by his withdrawal (see Index sub voce *entrepreneur*). The argument that the remuneration of the entrepreneur cannot be expressed by such simple formulæ is buttressed by an added note referring to the treatment of Risk in Mr. Keynes' *Treatise on Probability*.

As the regime of Competition is not universal, the theory of Distribution requires also the consideration of monopoly, in particular of two-sided monopoly; as when a compact association of entrepreneurs confronts a solid trade union. Is the ideal of distribution in such cases the arrangement which would result from an imaginary perfect competition, or rather a reasonable compromise rendered possible by a better mutual understanding, an enlarged sympathy?]

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Distribution is the species of Exchange by which produce is divided between the parties who have contributed to its production.[1] Exchange being divided according as both, or one only, or neither of the parties have competitors, Distribution is

[1] This definition, if not made more specific, includes some kinds of International Trade, just as the generic definition of International Trade includes some kinds of Distribution. See II. 5, 19.

similarly divided. The case in which both parties have competitors will here be first and principally considered.

The simplest type of this distributive exchange would be of a kind which is effected once for all, without reference to a series of future productions and exchanges. For example, to adapt an illustration used by Mr. Henry George,[1] let it be supposed that on a particular occasion each out of a number of white men hires one or more black men to assist in catching seals, on the agreement that each white man shall give his black assistants a certain proportion of the take, the terms having been settled in an open market in which any one white is free to bid against any other white and any one black against any other black. A conception more appropriate to existing industry is that each white agrees to pay in exchange for a certain amount of service a definite quantity of produce, not in general limited to the result of a particular operation. On a particular day less seal may be taken than the employer has agreed to give the employee for the day. In this case, even if payment is not made till the end of the day, the employer must pay for help on a particular day in part with seal caught on a previous day. He must pay altogether out of past accumulations when payment is made before the work is done. When the employer agrees to pay a definite amount, he cannot expect to gain on each day's transaction, but on an average of days.

This example is suited to illustrate some general properties of Exchange which attach to Distribution as a species of Exchange. Such are the laws which connect a change in the supply or demand upon one side of the market with a change in the advantage resulting from the transaction to the parties on either side. Thus, competition on both sides being presupposed, a decrease of supply in a technical sense of the term on the one side is, *ceteris paribus*, universally attended with detriment to the other side, but is not universally attended with detriment to the side on which the supply is decreased.[2] Accordingly, a limitation of supply on one side may be advantageous to that side, though not to both sides. The case of Distribution compared with Exchange in general in respect to such limitation of supply has only this peculiarity,—that the danger of this policy defeating itself is in the case of Distribution specially visible and threatening. There is an evident limit to what the black man dealing with the white man can get in exchange for a certain amount of his service;

[1] *Progress and Poverty*, Book I. chap. iii.
[2] See II. 8, 35.

namely, the total product which that service utilised by the white man will on an average produce. To be sure, there is here but a case of the general principle that no one will give more for a thing, whether article of consumption or factor of production, than the equivalent of its total utility to him, which total diminishes as the quantity of the commodity is reduced. But this limit is less liable to escape attention when it is fixed by the material conditions of production rather than by the desires of consumers. Conspicuous warning is given to parties in the position of our black men not to attempt to benefit themselves by a considerable reduction in their supply of service; for, though they might possibly obtain a larger proportion, they would probably obtain a smaller portion, of the average product. The laws which have been stated and other general laws of Exchange are equally true in more complicated cases of Distribution.

So far, we have supposed only a single factor—the service of the black man, or, more generally, the factor $\beta$—offered by the competitors, say, $B_1$, $B_2$, etc., in exchange for some of the produce $\alpha$ offered by the competitors, say, $A_1$, $A_2$, etc. Let us now introduce other kinds of factors, $\gamma$, $\delta$, etc. And let us no longer suppose payment to be made by parties of the type A, in the kind of commodity which is produced, namely, $\alpha$. A more concrete conception is that, besides the group A, B, C, D, there is another and another group,—$A'$, $B'$, $C'$, $D'$; $A''$, $B''$, $C''$, $D''$;—where each capital letter typifies a set of competing individuals. It may be supposed that each A purchases out of the finished product that he turns out—namely, $\alpha$—portions of the products $\alpha'$, $\alpha''$, etc., which he distributes according to the law of supply and demand among parties of the type B, C, D. In fine, each A may pay for the factors of production altogether in some one product, $\alpha'''$,—"*numéraire*," as happily conceived by M. Walras, or, less generally, money,—which the purveyors of the factors can exchange for the articles which they want. These articles need not be all commodities ready for consumption: some of the parties may care to purchase factors of production wherewith to play the rôle which has been assigned to A.

Having now obtained a general idea of the machinery by which distribution in a regime of competition is effected, let us go on to consider in more detail the parts of the mechanism. And, first, of the party that takes factors of production in exchange for products or the means of purchasing the same, the party above represented by the white man and labelled A. The

functions of this party may be investigated by an ancient method which Sidgwick has proposed to rehabilitate [1] for the purposes of modern economics,—the search for a definition. What is an entrepreneur? Amid the diversified combinations of attributes which the industrial world presents—innumerable as the varieties in which vegetable nature riots—we ought to fix certain characters agreeably to the rule laid down by Mill under the head of Definition by Type. "Our conception of the class" should be "the image in our minds which is that of a specimen complete in all the characteristics." [2] *Four* such type-specimens may be distinguished, ranged in a descending order according to the extent of functions ascribed to the entrepreneur. There is, *first*, the party whom the classical writers designate as the Capitalist, "who from funds in his possession pays the wages of the labourers, or supports them during the work; who supplies the requisite buildings, materials, and tools, or machinery; and to whom, by the usual terms of the contract, the produce belongs to be disposed of at his pleasure." [3] This party will here be considered as devoting his care and savings to a single business. There is, *second*, the entrepreneur as portrayed by the late President Walker, "not an employer because he is a capitalist, or in proportion as he is a capitalist." [4] There is, *third*, the party to whom Mr. Hawley would wish to restrict the term "entrepreneur," [5] the man who undertakes risks, of which class the most prominent, though not the only, species is the investor in joint stock companies.[6] *Fourth*, at the extreme degree of tenuity, is the entrepreneur who makes no profit. It might seem, indeed, as if this class did not call for special treatment, as differing only in the amount, not in the kind of remuneration. A fig tree which bears no fruit is not therefore a tree of a distinct species.

[1] *Political Economy*, Book I. chap. ii. § 1.

[2] *Logic*, Book III. chap. vii.

[3] Mill, *Political Economy*, Book II. chap. xv. § 1.

[4] *The Wages Question*, p. 228.

[5] *Quarterly Journal of Economics*, Vol. VI. (1892) p. 283; VII. p. 459 *et seq.*; XV. p. 77 *et seq.*

[6] Compare Mangoldt, *Unternehmergewinn*, pp. 41–43. A person who does not work, "wie der stille Gesellschafter, hört darum nicht auf, wahrer Unternehmer zu sein." This type is the limiting case, short of which the trouble of management in various degrees is combined with what Mr. Hawley calls "the irksomeness of risk." As Professor Taussig says, "The corporation of modern times presents all possible varieties of the relation between active manager and idle investor. Nominally, the stockholders are a group of associated active capitalists. Practically, they range from shrewd managers to the most helpless of inactive investors." *Quarterly Journal of Economics*, Vol. X. (1895) p. 83. *Cp.* Marshall, *Principles of Economics*, Book IV. chap. xii. §§ 8 and 9.

The horse which the Scotchman its owner had just trained to live upon a minimum, when the animal unfortunately died, was not therefore a new variety of the equine genus, requiring mention in a treatise on Natural History. However, as imposing theories have been connected with this last category, it comes within the scope of the present inquiry.

As our aim in comparing definitions should be, as Sidgwick says, "far less to decide which we ought to adopt than to apprehend the grounds on which each has commended itself to reflective minds,"—the hunt for a definition being followed not so much for the sake of the quarry as of the views which are incidentally presented,—let us go on to consider the principal propositions which the several conceptions are adapted to bring under our notice. In this inquiry much assistance will be obtained from a series of articles on cognate subjects in the *Quarterly Journal of Economics*,[1] which forms a sort of economic symposium.

The *first* definition is particularly suited to inquiries in which the parties who are in the habit of saving are contrasted as to their actions and interests with the parties who do not save,—approximately, the working classes. Specimens of such inquiry may be found in the fifth chapter of Mill's first book, and in Professor Taussig's important article on "The Employer's Place in Distribution."[2] It sounds paradoxical to add that the classical conception is not particularly adapted to illustrate the Ricardian theory of rent. But the definition of the capitalist above given is not easily reconciled with the received representation, that the capitalist's remuneration is equal to the number of doses which he lays out, multiplied by the remuneration of the last dose, the ordinary rate of profit. For, as Sidgwick argues, there is no adequate reason for expecting that "remuneration for management" as well as interest should tend to be at the same rate for capitals of different sizes.[3] Doubtless, the proposition is accurate enough to support the practical consequences which have been deduced from it. But, while fully admitting this, one may still agree with Sidgwick that "even Mill's exposition" is "highly puzzling." For the idea of an economic person laying out doses up to the margin and obtaining the remuneration equal to the number of doses multiplied by the marginal productivity of each dose is only proper to the case in which the doses are for sale.

[1] References to the series up to November, 1900, are given in the *Quarterly Journal of Economics*, Vol. XV. p. 75.

[2] *Quarterly Journal of Economics*, Vol. X. p. 72.

[3] *Political Economy*, 3d edition, Book II. chap. ix. § 3. *Cp.* chap. ii. § 8; and below, p. 21.

But it is only in the conditions proper to our third definition that doses of capital are put on a market in exchange for profit. Perhaps the classical writers, having an eye to practice and not restricted by a sharp definition, often tacitly introduce the supposition that it is open to the "capitalist" to take part in some other business besides his own.[1]

The classical formula for surplus may be employed along with our *second* definition if we use the phrase "amount of outlay multiplied by average rate of return" to designate the amount which the entrepreneur of the Walker type pays in the way of interest from year to year to those who have lent him the means of carrying on his business. The surplus, according to this conception, will include not only the landlord's rent, but also the entrepreneur's net income. The portion of this surplus which accrues to the entrepreneur is not given by any simple formula. The conditions by which it is determined may be considered under two heads, corresponding to Cairnes's categories,—commercial and industrial competition. This distinction becomes clearest when, in conformity with the division of employments, we conceive different occupations to be separated by great gulfs, so that they who would pass from one to the other must make a complete, or at least a considerable, change in their business arrangements.[2] In virtue of the first kind of competition the entrepreneur endeavours to make the best possible arrangements within the occupation which he has chosen. In virtue of the second kind of competition he endeavours to choose the occupation which will afford to him the greatest net advantage.

His motive under the first head may be understood by likening

[1] *Cp.* Mill on various employments of capital, *Political Economy*, Book II. chap. xv. § 1, par. 4.

[2] See note to the present writer's Address to the British Association, Section F, 1889 (a, vol. ii.), which, written before the publication of Marshall's *Principles of Economics*, does not sufficiently emphasise the "principle of continuity." It may be observed that the two kinds of competition involve respectively two mathematical operations, the determination of a maximum, and of the greatest among maxima. There is the distinction between finding the top of a hill and finding the highest hill-top. The demarcations between the two species of competition and between the two mathematical operations are not coincident, so far as an entrepreneur, without leaving his business, may introduce considerable and, so to speak, integral changes in its organisation, in accordance with the "principle of substitution" (Marshall). This principle seems to cover both the species of competition and both the mathematical operations. Doubtless, it is convenient to have a term applicable to every method by which maximum advantage is sought. Among such methods ought, perhaps, to be placed the *calculus of variations*, where the "margin of profitableness" is considered as "a sort of boundary line, cutting one after another every possible line of business organisation." *Principles of Economics*, Book VI. chap. vii. § 7, 4th edition.

him to a monopolist who does not control the prices of the factors of production, nor yet the price of the product, the latter being fixed by a maximum law, or, rather, the case being that in which the monopoly is just becoming extinct, as Cournot would say, by the introduction of competitors, so that this entrepreneur can no longer sensibly alter at will the price of the product. Under such circumstances each entrepreneur will vary all the variables under his control up to the margin at which his own advantage becomes greatest. If he or we be content with a rough estimate of this advantage, it may be measured by the difference between his incomings and outgoings. His incomings may be regarded as the product multiplied by the price thereof, the amount of the product depending in some definite manner on the amounts of the factors of production which are employed.[1] The outgoings may be regarded as a sum of terms, each of which is the amount of a factor of production multiplied by its price.[2] It follows[3]

[1] Some *function* of the amounts.

[2] Or, rather, the *accumulated* price, in the sense explained by Professor Marshall (*Principles of Economics*, Book V. chap. iv. § 2, p. 432, 4th edition): "Looking backwards, we should sum up the net outlays, and add in accumulated compound interest on each element of outlay." Compare note xiv. of his mathematical Appendix. Abstraction was made of this sort of correction in the British Association Address to which reference has been made. For instance, it was tacitly assumed that the entrepreneur might have as much labour as he could pay for (at a prevailing rate of wages) at the time when the value of the finished product was realised. Professor Barone has pointed out the need of greater accuracy and a means of obtaining it by employing his remarkable conception of "capital of anticipation." *Giornale degli Economisti*, February, 1896.

[3] Marshall, *Principles of Economics*, Book VI. chap. i. § 8, 4th edition. Mr. J. A. Hobson's criticism of this doctrine exemplifies the difficulty of treating the more abstract parts of Political Economy without the appropriate mathematical conceptions. An elementary discipline in the differential calculus would have corrected the following passage and its context: "In order to measure the productivity of the last dose of labour, let us remove it. The diminution of the total product may be 8 per cent. This 8 per cent., according to Marshall's method, we ascribe to the last dose of labour. If now, restoring this dose of labour, we withdrew the last dose of capital, the reduction of the product might be 10 per cent. This 10 per cent. is regarded as the product of the last dose of capital. Similarly, the withdrawal of the last dose of land might seem to reduce the product by 10 per cent. What would be the effect of a simultaneous withdrawal of the last dose of each factor? According to Marshall's method, clearly 28 per cent. But is this correct?" *The Economics of Distribution*, p. 146. Quite correct, if in the spirit of the differential calculus we understand by dose an increment as small as possible, not as large as the objector pleases. He goes on: "Put the same experiment upon its broadest footing, and the overlapping fallacy becomes obvious. Take the labour, capital, and land as consisting of a single dose each; now withdraw the dose of labour, and the whole service of capital and land disappears. Is the destruction of the whole product a right measure of the productivity of the labour-dose alone?" (*loc. cit.*, p. 147). Imagine an analogous application of the differential calculus in physics, "put upon its

that in a state of equilibrium the increment of value produced by the last increment of a factor is just equal to its price. "The *marginal* shepherd . . . adds to the total produce a net value just equal to his own wages." [1]

So far supposing the entrepreneur's work to be a constant quantity. In a more exact estimate the quantity which the entrepreneur seeks to maximise is the utility to be derived from his net income *minus* the disutility incident to its production. From this consideration it follows that the increment of utility due to the increment of product which is produced by the last increment of entrepreneur's work is just balanced by the increment of disutility due to that work.

To this condition is superadded the tendency towards equal net advantages in different occupations, resulting, as Professor Marshall has shown, not so much in the equal advantageousness as in the equal attractiveness of different occupations. The remuneration of the entrepreneur thus corresponding to his services may be classed along with the remuneration of the workman as "earnings," from a certain point of view, which is doubtless proper to the publicist and philosopher. As Mangoldt points out, "the circumstance that certain services do or do not attain a market price" does not "essentially alter the measure of their compensation." But there is another point of view which is proper to those who study the mechanism of distribution. As Professor Taussig well observes, "The cobbler who works alone in his petty shop gets in the main a return for labour as much as the workman in the shoe factory"; but "with regard

broadest footing," an objector substituting $x$ wherever a mathematician had used $dx$ or $\Delta x$!

[1] It being assumed that the function expressing the product in terms of the factors of production is such that for the values of the variables with which we are concerned the net income of the entrepreneur may be a maximum, let P be the amount of the product, $\pi$ its price, $a$, $b$, $c$, amounts of factors of production, $p_1$, $p_2$, $p_3$, etc., their respective prices—their actual prices—for a first approximation, their *accumulated* prices for a more accurate statement. The net income of the entrepreneur may then be written (abstraction being made of the entrepreneur's own effort) $P = \pi f(a, b, c) - p_1 a - p_2 b - p_3 c$. In order that this expression may be a maximum, the law of decreasing returns must hold in the *first* of the two senses elsewhere distinguished (below, p. 67 and p. 152). The condition must still be postulated when account is taken of the entrepreneur's subjective feelings,—effort and sacrifice in the way of production balanced by satisfaction immediate or prospective in the way of consumption. Nor is the case essentially altered when account is taken of the possibility (noticed by Professor Pareto, *Cours*, Art. 718) that the factors are not independent. Suppose that the amount of labour must always be in proportion to, or on any definite function of, the amount of land. Then, eliminating one of these quantities, we may treat the other as independent.

to the machinery by which distribution is accomplished he [the cobbler] belongs in a different class from the hired labourer." [1]

The tendency to equality of net advantages of course only exists with respect to positions between which there is industrial competition. Accordingly, if the union in one person of natural abilities and money constitutes him a member of a "non-competing group," there is no presumption that the remuneration of such an entrepreneur will be exactly equal to the interest which he might have obtained by lending his money plus the salary which a person of his ability could command as a hired manager. There exists an excess above that sum, corresponding to what Mangoldt calls *Unternehmergewinn*. There may be excesses somewhat similarly caused by different degrees of ability and resources; the various "rents" enumerated by Mangoldt, which, as he observes, tend to diminish with the progress of society, so far as education becomes more diffused and it becomes easier for persons properly qualified to obtain the use of capital.

Some additional light on the functions of the entrepreneur may be obtained by comparing the profits in businesses of a different size. Suppose (for the sake of the argument) that the work and worry of the "boss" do not increase [2] with the scale of operations, how is the equality of net advantages which theory leads us to expect brought about? *Ceteris paribus*, might we not expect the entrepreneur's residue to be larger in the large industries? [3] The answer seems to be that, as equilibrium is approached under the joint influence of Commercial and Industrial Competition, the amounts of the factors [4] are so varied as to fulfil the condition that equal efforts and sacrifices on the part of the entrepreneur are attended with equal remuneration.[5] This equality is irrespective of identity in the relation between factors and product.[6] It may exist whether that identity is supposed to

[1] *Quarterly Journal of Economics*, Vol. X. (1895) p. 88. Professor Taussig goes on, "For an understanding of the machinery by which distribution is accomplished in modern times, the classification of sources of income should thus be different from that to be adopted for an explanation of the fundamental causes."

[2] That the trouble does not increase proportionately would be a more concrete supposition. As Sidgwick says, "Though it is more troublesome to manage a large factory than one half the size, it can hardly be twice as troublesome." *Political Economy*, Book II. chap. ix. § 3.

[3] *Cp.* Marshall, *Economics of Industry*, Book II. chap. xii. § 4, 1st edition.

[4] The factors generally, and sometimes also the form of the function expressing the quantity of the product in terms of the quantities of the factors used, the function designated $f$ in note to p. 20.

[5] The equality is that of an ordinary equation, not an identity.

[6] The function which expresses the amount of the product in terms of the factors (including entrepreneur's work).

be present between industries of different sizes or, as in general to be supposed, there is no identity in the relation between factors and product for different individuals and industries.

The sort of adjustment thus postulated may be illustrated by a more familiar kind of surplus, that which accrues to the landlord according to the received theory of rent. Let there be a homogeneous tract of land equally adapted to the cultivation of wheat and barley, owned by a set of competing landlords, who accordingly obtain an equal rent per acre whether wheat or barley is to be grown thereon.[1] Now let a tax be imposed on the rent of land used for growing barley. There must result a new equilibrium, in which it remains true that owners of homogeneous land obtain equal rent per acre for whichever purpose used, and that cultivators of wheat and barley obtain, *ceteris paribus*, equal profits. These conditions can be fulfilled if the extent of the land applied to the cultivation of wheat is increased while the intensity of cultivation is diminished, and contrariwise for barley the extent is diminished and the intensity increased. This proposition holds good whether or not the relation between outlay and product [2]—corresponding to the shape of the curve in the illustration which Professor Marshall has made familiar [3]—is supposed identical for wheat and barley, and even if the cultivator seeking the greatest possible profits is able to vary that relation in accordance with the "law of substitution." It is here assumed that the case of manufacture is not so different from agriculture, but that an analogous adjustment of "margins" must be considered to take place between large and small businesses under the conditions specified, and generally between different industries where industrial competition acts.

A similar adjustment must be postulated when we entertain the *third* definition of entrepreneur, and consider competing investors in the stock of companies which may at first be supposed equal in respect of risk, though not in size. The competitors being free to invest units consisting, say of £100 or less in any kind of business (of the given riskiness), large or small, it follows that a return to a dose anywhere invested tends, *ceteris paribus*, to be the same.[4] This result, which is by no means a deduction

[1] Compare II. 78.

[2] The function expressing the product in terms of the outlay.

[3] *Economics of Industry*, 1st edition, p. 83. *Principles of Economics*, 4th edition, p. 232.

[4] Accordingly, in order that equilibrium should be stable in this regime, investment in each industry ought to be pushed up to a point at which the law of decreasing returns is fulfilled in its *second* sense,—that the rate of total cost to total product increases with the increase of product.

from the general formula considered under our second head, may be supposed to be brought about by an adjustment of margins of the sort which has been explained.

Now at length the Ricardian theory of rent as ordinarily stated becomes exact,—the payment for land rented by a joint stock company ought to be just the difference between the returns (after capital has been replaced and labour paid) and the amount of capital laid out, multiplied by an average rate of profit.

Though the class of shareholder is the principal, it is not the only species, of the third kind of entrepreneur, if defined so as to include all risk-takers. As Mr. Hawley observes,[1] workmen take some risk, entrepreneurs who have no capital of their own run the risk of not being paid for their trouble. Enterprise may be taken as the essential attribute of a wide class entitled to a share in the national dividend along with the purveyors of land, labour, and capital. It does not seem to be a fatal objection that enterprise is hardly to be found in the concrete, separate from other factors of production. As Mr. Hawley replies,[2] labour and waiting, the attributes of familiar classes, are not to be found in abstract purity.

To some there may seem a more serious scruple: whether the undertaking of risk does even in thought constitute a fourth factor, whether the distinction between interest and the reward for risk is radical. It is all very well for Jevons to distinguish by different coefficients, $p$ and $q$, the depreciation of future goods due to uncertainty and to remoteness. But, since the distant pleasure is always uncertain, can we really disentangle the two causes of depreciation?

Fortunately, these questions of logical definition and psychological analysis do not affect the important lessons respecting the participation of risk which have been taught by Professor J. B. Clark,—"that a corporation can run risks which the individual could not with prudence," that by forming corporations "we reduce the initial terrors of business enterprises."[3] It is an exemplification of the old maxim not to put all one's eggs in one basket. If a hundred persons are carrying each a hundred eggs, each independently running the risk of tripping and by the loss of all or many of his eggs being exposed to great privation, this great danger will be averted, this chance of great disaster will be commuted for a somewhat higher probability of a much more

[1] *Quarterly Journal of Economics*, Vol. VII. (1893), p. 470.
[2] *Ibid.*, Vol. XV. (1900) p. 78.
[3] "Insurance and Business Power," *Ibid.*, Vol. VII. (1892) p. 40, *et seq.*

easily borne loss, if each person carries only one of his own eggs and one belonging to each of the rest, the total to be redistributed at the end of the journey to market or after sale.

It is noticeable that in Professor Clark's nomenclature this risk is borne by the capitalist. "The hazard of business falls on the capitalist." "Business repays men not only for their labours, but their fears." But this repayment is "not a part of mercantile profit": it is realised by the capitalist "as such." Admitting a real remuneration for risk, while giving a different name to the recipient from that which others have preferred, Professor Clark is perhaps not committed to the paradox which Mr. Hawley would affix upon the conception of the entrepreneur with vanishing profits,—our *fourth* species.*

"To eliminate profit, wholly static conditions must be more absolute. . . . There must be a cessation of all variations due to the changeableness of the environment due to fire, lightning, hail. We must imagine industrial society in the static condition as an automatic machine, . . . working without friction in an absolutely unchangeable environment." [1]

This idea of perfect tranquillity is certainly inappropriate to the troubled world in which we live. "Things are always finding their level," like a fluctuating and, in nautical phrase, "confused" sea. The oscillating character of the waves is quite consistent with a gradual change of level, as when the tide is flowing. It is a legitimate conception, familiar in statistics, to regard a phenomenon as hovering about an average, even though that average is known to be changing. Let the great tidologist calculate the dynamics of the flow, but let him not convey the impression that but for the action of this flow there would be the level of the proverbial mill-pond. Very probably, however, Professor Clark would recognise the continuance of risk not involving secular progress,—due to unpredictable weather or credit cycles, for example,—but would regard the remuneration for undergoing such risk as accruing to the "capitalist as such" rather than, with Mangoldt and others, as a part of the entrepreneur's gain. With regard to other elements of remuneration it is more doubtful whether Professor Clark would accept Mangoldt's statements as to the permanence of the entrepreneur's gain,—statements which read with their context, and attention being paid to Mangoldt's terminology, deserve much consideration.

* See the appended note (p. 59), referring to the observations on Risk in Mr. Keynes' *Probability*.

[1] *Quarterly Journal of Economics*, Vol. XV. (1900) p. 91.

We must suppose the existence of undertaker's gain [*Unternehmergewinn*],—otherwise what object has the entrepreneur to increase his business? (substance of p. 50).

The undertaker's gain (*Unternehmergewinn*) is "not simply something transitory," but a "permanent species of income" (p. 51).

"The undertaker's remuneration [*Unternehmerlohn*] preserves its position, though in a limited form" (p. 105. *Cf.* p. 169).

Perhaps Professor Clark would be satisfied with the "limited form" of the remuneration and the disappearance of certain other elements.

It is always pleasant to believe that one's differences with high authorities are only verbal. This satisfaction may now be enjoyed with respect to M. Walras's doctrine that the entrepreneur makes neither gain nor loss. Professor Pareto [1] has made it clear that, as the object of the entrepreneur is to procure the greatest amount of satisfaction, so his income is not to be considered as *nil*, in the ordinary sense of the term. Rightly interpreted, the doctrine that "the entrepreneur makes neither gain nor loss," taken in connection with the "coefficients of production," appears to cover all the conditions of equilibrium, both those which are involved in what Cairnes called "industrial competition" and those which would be satisfied even if we made abstraction of the tendency to equal advantages in different occupations.[2] But, while we accept the ideas, we are not bound to adhere to the words of a master; and the expression in question may be objected to on several grounds which will repay examination. It is violently contrary to usage; it lends itself to a dangerous equivoque; and it has led distinguished economists to paradoxical conclusions.

No amount of authority and explanation can make it other than a strange use of language to describe a man who is making a large income, and striving to make it larger, as "making neither gain nor loss." There is an oddity about the phrase which recalls the use of "gratis" by Sir Murtagh's lady in *Castle Rackrent*: "My lady was very charitable in her own way. She had a charity school for poor children where they were taught to read and write gratis, and where they were kept well to spinning gratis for my lady in return."

A more serious objection is that the term "making neither

[1] *Cours d'Économie Politique*, passages referring to "entrepreneur." [But see II. 378 and 469.]

[2] *Cp.* above, p. 18.

gain nor loss" has to be used in two different senses almost in the same breath. It is a sufficiently difficult lesson for the plain man to learn that the maximum of income which the entrepreneur aims at realising is zero. But the difficulty is doubled when he comes to learn—as he must in dealing with a maximum problem—that the increment to that income due to the last increment of any factor of production is also zero. There is apt to arise a confusion between conditions belonging to the total and to the marginal quantity,—an ambiguity of a kind which has before now proved detrimental in economics.[1] A hasty reader of Professor Walras might suppose that it was intended to affirm that the entrepreneur made neither gain nor loss *at the margin*: whereas the meaning is, rather, that nothing remains to be distributed—on an average and apart from oscillations—after that the entrepreneur has paid a normal salary to himself.[2]

The implication that the remuneration of entrepreneur labour may be treated like that of any other labour presents some difficulty. It is the one obscure topic in Professor Barone's brilliant studies on Distribution.[3] His observations deserve to be quoted at some length. He first (in a note on p. 132) announces as true in a particular case, what is here regarded as true in general, that "there must be left to the entrepreneur's profit (*profitto dell' impresa*) the differentiating character of 'residual claimant'; and nothing else can be said but that profit is formed by the difference between the entire product and the remunerations of the various factors corresponding to (*ragguagliate*) their respective marginal productivities." But Professor Barone regards this enunciation as only provisional. He promises to show in a later section that "with the increase in the number of the competing entrepreneurs the profit of the undertaking tends to lose more and more the character of residual claimant, and tends to conform to that of the law of marginal productivity."

In the later section he says:—

"If on the market there is only one entrepreneur, Titius, and if he does not monopolise the product, that is, if he in the

[1] Mill's hesitation between equal sacrifice and least sacrifice as the criteria of taxation may seem due to a confusion of this kind, as pointed out by the present writer in the ECONOMIC JOURNAL, 1897. (Cp. *Mathematical Psychics*, p. 118.) Mill's ambiguity had already been noticed by Professor Carver in his article on "The Ethical Basis of Distribution" in the *Annals of the American Academy for* 1895, p. 95.

[2] *Cp.* Pareto, *Cours*, Art. 87, "his salary as director of the enterprise being comprised in the expenses of production"; and the similar expressions of Professor Barone, quoted below.

[3] *Giornale degli Economisti*, February, 1896.

management of his business arranges [*fa in modo di*] to obtain not indeed the greatest monopoly profit, but the greatest profit obtainable in a regime of free competition, . . . his profit will be [a surplus indicated by a figure which is not here reproduced]. But, if there is an entrepreneur Caius capable of entering into competition with the preceding, . . . the profit of Titius will be reduced below what he had when he was alone on the market. And, if there is a third employer also capable of entering into competition with the first two, the profit of Titius will be reduced still more. The more the number of employers increases, the more there is a necessary tendency to a *limiting state* in which all the employers who continue to produce have a remuneration which, like that of any other labour, satisfies the condition that the marginal disutility [*penosità*] of the same labour [*medesimo*] shall be equal to the marginal utility of the returns which that labour procures, *and not more than this.* And, since it is this equality which characterises the return to labour, it follows (*ne viene*) as a legitimate consequence that in this limiting state the remuneration of the entrepreneur may be treated like the remuneration of any other species of labour."

The fact that wages are usually paid in advance is not to the point, as Professor Barone very properly observes. He proceeds :—

"These considerations seem to me to prove to demonstration how profound and correct is Walras's conception of an entrepreneur who under the conditions postulated makes neither gain nor loss after having paid himself (or others, it is indifferent which) the remuneration of the labour of direction and conduct of production. And, if it is no wonder that this conception should not be comprehended by economists who have really very vague ideas of quantity, it is absolutely astounding that the conception should have been also made the subject of criticism by other economists to whom the notions of quantity are quite familiar. . . . I frankly must confess myself absolutely incapable of understanding how any difficulty whatever can arise as to the validity [literally, the affirmation] of this conception, which is indeed most simple."

Having called once more attention to the abstract character of the conditions, Professor Barone reiterates :—

"In such conditions the law of marginal productivity extends to the remuneration of the entrepreneur; and, after having remunerated all the factors (the work of the entrepreneur included) in proportion to their marginal productivity [with a

discount corresponding to the time elapsing between the service and the product], there remains no undistributed residue."

If there could be any doubt about the meaning of this thesis, it would be removed by the unequivocal language of symbols employed in the Appendix,[1] where, by way of illustration, the labour of the entrepreneur is expressed by the total number of hours of work that he devotes to the business.

Upon this it may be remarked that the last state of Titius, after Caius and the rest have entered as competitors, seems identical with the case of "extinct" monopoly which was above [2] adduced, in order to exhibit the motives of the entrepreneur. As there appears, both before and after the competitors have entered the remuneration of the entrepreneurs, in Professor Barone's phrase, "satisfies the condition that the marginal disutility of the labour shall be equal to the marginal utility of the return which that labour procures." But neither before nor after the competitors have entered is there any reason for regarding the remuneration of the entrepreneur as the product of the number of doses (*e. g.* hours worked) and the marginal productivity of a dose (multiplied by a coefficient depending on the length of the productive process [3]). It is only with respect to factors of production which are articles of exchange that the proposed law of remuneration, the "law of marginal productivity," is fulfilled in a regime of competition. Thus, in our typical example of black men assisting white men to catch seals,[4] what the black man gets in a perfect market is an amount of seal equal to the number of units of service which he supplies, multiplied by the quantity of seal for the sake of which he is just induced to offer an additional unit of service, the unit employed being a small quantity. Likewise, what the white man gets in exchange is an amount of service equal to the amount of seal which he distributes to the black man, multiplied by the quantity of service for the sake of which he is just induced to offer an additional unit of produce. If the amount of service rendered may be taken as the measure of the black man's labour (or of some other factor of production supplied by him), the proposed law holds good for his share of the distributed produce. But, as the amount of produce given by the white man in exchange for services cannot

[1] *Loc. cit.*

[2] Above, p. 19.

[3] *Cp.* note 2 to p. 19 above; but remark that the correction proposed by Professor Barone for the effect of time is not identical with Professor Marshall's *accumulation* of price.

[4] Above, p. 14.

be taken as the measure of his work, the proposed law does not hold for his share of the distributed produce.

This discussion will appear otiose to the economists who are not conversant with the science of quantity. The proposition that the remuneration of the entrepreneur is equal to the amount of his work multiplied by its marginal productivity will be interpreted by them as signifying simply that he will get more, *ceteris paribus*, the more work he does and the greater the addition to the produce which he would effect by doing a little more work. For them a *product* will do duty for a function of two variables which increases with the increase of either variable. But this easy interpretation is not open to mathematical economists. They must be aware that the formulæ in question affirm something more than the simple truth just stated. If nothing more than that simple truth can be deduced from the theory of Exchange, it ought not to be a matter of surprise that the "law of marginal productivity" applied to the entrepreneur should be challenged by those who affect mathematical precision.

The law of marginal productivity, then, is not fulfilled in the sense that the portion of the national dividend accruing to entrepreneurs is a sum of terms each of which is the product of an entrepreneur's work reckoned in hours, or similar doses, and the marginal productivity of a dose (multiplied by a certain coefficient [1]). Let us see whether the law is fulfilled when we take a larger dose, the total work of an entrepreneur. The law will then be fulfilled if the net gains of any entrepreneur tend to be equal to what society would lose if he were removed. Can this be generally affirmed? Let us look at the typical case of distribution between whites and blacks above [2] instanced. It may be granted that the white entrepreneur does not normally obtain more than he adds to the common stock. For otherwise the society would gain through his removal, his black assistants either hunting by themselves or being taken on by other entrepreneurs. And neither of these suppositions is possible in a state of equilibrium; for, if either were possible, it would have been already brought about by the free play of self-interest, in a regime of competition. The gain of a white man, then, cannot be greater, but where is the proof that it cannot be less, than the loss which would be occasioned to the society by his removal?

Such a proof might be forthcoming if the white men were not, as hitherto supposed, genuine entrepreneurs, but managers acting under entrepreneurs of our third species, the stockholder.

[1] Above, p. 19, note. [2] Above, p. 14.

The income of the managers will fulfil the marginal law of productivity if the new entrepreneurs are conceived as competing against each other in such wise as to bring about the result that no manager earns more or less than what he adds to the profits of his employers. The income of the new entrepreneurs also fulfils the law; for the remuneration of this species of entrepreneur—unlike that of entrepreneurs in general—is proportional to the amount of the factor which they contribute,—namely, capital invested.[1]

The affinity between entrepreneurs and salaried managers in modern industry supplies the missing link for the general proof of the new law. For, normally, it may be presumed that an independent entrepreneur (of our second species) does not make less (in addition to the profits that he makes or might have made by investing in some other business money of his own) than a manager of like abilities. And perhaps he does not make much more. The difference is possibly small,[2] probably diminishing, certainly difficult to verify statistically, perhaps hardly worth fighting about. Interpreted cautiously, the law holds good approximately. If the remuneration of the manager, like that of the "marginal shepherd," is just equal to the amount that he produces, then the remuneration of the entrepreneur is not very different from the amount that he produces. But, if the law of marginal productivity is fulfilled for the manager only while we consider doses less than his total work, say hours of work, then the law is fulfilled for the entrepreneur only so far as it is presumed from the similarity in nature and habits between the manager and entrepreneur that, when the total remuneration of each is nearly the same, the amount of work and its marginal productivity are not very different.

According to the interpretation which has been suggested, the new law of distribution would be fulfilled by an adjustment of the quantities involved,[3] the amount of each factor, not simply in virtue of the relation which subsists between the product and the factors of production.[4] The sense in which the law is fulfilled is otherwise conceived by a distinguished mathematical economist, Mr. Wicksteed, who regards the law as following from "the

[1] Above, p. 23.

[2] Mainly and apart from "rents" of the order of quantity called by Mangoldt *Unternehmerlohn*.

[3] *Cp.* p. 20, above.

[4] The form of a function such as that represented by $f$ in a preceding note (p. 20), or rather what that function becomes when the work of the entrepreneur enters as a variable.

modern investigations into the theory of value," [1] and seems to treat it as a clue whereby to investigate the nature of the relation between the product and the factors of production, including the work of the entrepreneur.[2] In fact, he finds that the product depends upon the factors by a relation which mathematicians designate a "homogeneous function of the first degree." [3] This is certainly a remarkable discovery; for the relation between product and factors is to be considered to hold good irrespectively of the play of the market: "an analytical and synthetical law of composition and resolution of industrial factors and products which would hold equally in Robinson Crusoe's island, in an American religious commune, in an Indian village ruled by custom, and in the competitive centres of the typical modern industries." [4] There is a magnificence in this generalisation which recalls the youth of philosophy. Justice is a perfect cube, said the ancient sage; and rational conduct is a homogeneous function, adds the modern *savant*. A theory which points to conclusions so paradoxical ought surely to be enunciated with caution.

To sum up this criticism, as Distribution is a species of Exchange, it seems undesirable to employ a phrase so foreign to the general theory of Exchange as the dictum that one of the parties to an exchange normally gains nothing. Innocently used at first, such paradoxes are calculated to lead to confusion and misrepresentation.

A similar remark applies to another form of the gainless entrepreneur, involved in Walker's analogy between profits and agricultural rent.[5] Even on the simpler and provisional view

[1] *Essay on the Co-ordination of the Laws of Distribution* (1894), § 2, and prefatory note.

[2] The product being a function of the factors of production, we have $P = f(a, b, c, \ldots)$; and the form of the function is invariably such that, if we have $\pi = f(\alpha, \beta, \gamma, \ldots)$, we shall also have $\nu\pi = f(\nu\alpha, \nu\beta, \nu\gamma, \ldots)$ (*loc. cit.*, p. 4).

"Let the special product to be distributed ($P$) be regarded as a function ($F$) of the various factors of production ($A, B, C, \ldots$)" (*loc. cit.*, p. 8).

$$\frac{dP}{dA}A + \frac{dP}{dB}B + \frac{dP}{dc}C + \ldots = P$$

"under ordinary conditions of competitive industry" (*loc. cit.*, pp. 33–38).

[3] As pointed out by Professor Flux in his review of Mr. Wicksteed's essay, Economic Journal, Vol. IV. p. 311. In Mr. Wicksteed's notation the function $f$ must be of the general form $A\psi\left(\frac{B}{A} + \frac{C}{A} \ldots\right)$, where $\psi$ is an arbitrary function. See Forsyth, *Differential Equations*, Art. 189, or Boole, *Differential Equations*, chap. xiv., Art. 6.

[4] *Loc. cit.*, p. 42.

[5] As argued by the present writer in his Address to the British Association for the Advancement of Science, 1889, written before the publication of Professor Marshall's weightier judgment in the *Principles of Economics*.

which is confined to short periods and commercial competition, this form of expression has no advantage over the terminology proper to the general theory of Exchange. When we consider long periods and industrial competition, Walker's theory has the graver disadvantage of not distinguishing between rent and quasi-rent. It seems to be generally admitted that Walker's masterly portrait of the industrial captain was not improved by his representation of profits as rent.[1]

Having now considered the party that takes factors of production in return for products, or the proceeds thereof, let us look at the other side of the counter,—the triangular counter across which we may imagine the three factors of land and labour and capital to be exchanged, if we place in the interior of the triangle an entrepreneur of Walker's type, our second species, dealing with three parties in quick succession, and in some sense simultaneously.[2]

At the height of abstraction from which it is here attempted to survey the economic world, what appears the most salient feature in the transactions respecting *land* is the circumstance that the quantity of ground, or at least space,[3] is limited, not capable of being increased by human effort. From this property flow most of the general theories relating to the landlord's share in distribution,—that a tax on rent (proper) falls wholly on the land, that the remission of agricultural rent by landlords would not benefit the consumer,[4] and other propositions often connected with the formula that "rent does not enter into the cost

[1] Compare Mr. J. H. Curran's temperate criticism in his study on Walker (in Conrad's *Abhandlungen*).

[2] In the sense in which equations are called simultaneous.

[3] *Cp.* Marshall on "extension" as the "fundamental attribute of land." *Principles of Economics*, Book IV. chap. ii. p. 221 *et seq.*, 4th edition. Not even the enterprise of Boston, which converted marshes into the site of noble streets, can form an exception to the law so stated. But the more familiar statement is accurate enough. For, as Professor Bullock has said (at the banquet of the Massachusetts Single Tax League, 1902), "it may be safely contended that the additions which man can make to the land surface of the globe are so small as to be a negligible quantity when we compare land with the things that human labour places upon it."

[4] The received proposition is of the nature of a first approximation, as pointed out in II. 76. When the writer there observed that "there might be now required a higher rate of remuneration to evoke the same exertion from the cultivator," *et seq.*, he was not aware that he had been anticipated by the very first writer who stated the true theory of rent, James Anderson, who says that the only consequence of remitting rents "would be the enriching one class of farmers at the expense of their proprietors, without producing the smallest benefit to the consumers of grain,—perhaps the reverse, as the industry of the farmer might be slackened." *Enquiry into the Nature of the Corn-laws* (1777), p. 48, note.

of production." Some remarks on that time-honoured formula seem called for here. It would not be consistent to have complained of the expression that " the entrepreneur makes no gain " as perplexing and apt to mislead, however innocently used by high authorities, and to pass over in silence this dictum about rent, against which and in favour of which much the same is to be said. Certainly, it is supported by very high authority,—the authority not only of Ricardo and Professor Marshall, but also of Hume, who in the letter which he wrote to Adam Smith on the publication of *The Wealth of Nations* (the letter which, written a few months before Hume's death, may be considered his economic testament) says, " I cannot think that the rent of farms makes any part of the price of the produce, but that the price is determined altogether by the quantity and the demand." [1] On the other hand, it can hardly be denied that the dictum in question is calculated to obscure the truth that " land is but a particular form of capital from the point of view of the individual manufacturer or cultivator "; [2] that, as he doses land with capital and labour, so he doses capital and labour with land,[3] up to a margin of profitableness. And, in fact, the similarity of the factors of production from the entrepreneur's point of view does not seem to have been apprehended in all its generality by the classical writers. Thus Fawcett, who may be taken as a type, when explaining rent seems to posit the size of the farm as something fixed and constant.[4] J. S. Mill argues that " there is always some agricultural capital which pays no rent," [5] not noticing the counter-argument that there is a portion of land which pays no interest.[6] These imperfections belong now, it may be hoped, to past history. And yet that the description of rent as not entering into price is apt to prove misleading may be inferred from the many protests which eminent critics have raised against Professor Marshall's use of the time-honoured phrase.[7] Their criticisms attest the correctness of their own

[1] Burton's *Life of Hume*, Vol. II. p. 486.

[2] Marshall, *Principles of Economics*, Book V. chap. ii. § 5.

[3] The propriety of reversing the classical formula so as to make dose and patient change places is well expressed by Mr. Wicksteed, *Laws of Distribution*, p. 20.

[4] *Manual of Political Economy*, Book III. chap. iii.

[5] *Political Economy*, Book II. chap. xvi. § 4.

[6] As noticed by Professor J. B. Clark and other writers mentioned by Professor Fetter in the *Quarterly Journal of Economics*, Vol. XV., note to p. 436.

[7] See in particular Hobson's *Economics of Distribution*, chap. iv.; Fetter, " The Passing of the Old Rent Concept," v. and vii. (3), *Quarterly Journal of Economics*, Vol. XV. (1901); J. B. Clark, *Political Science Quarterly*, March,

views rather than their capacity of appreciating the views of others. What should we say of critics who should think fit to read Mill a lecture on the errors of the Mercantile system, because Mill had employed the terms "favourable and unfavourable" exchanges! To have attributed to Professor Marshall the very error which he by his doctrine of the "Margin-of-building" has done more than any other economist to obviate would be unpardonable if it were not excused by the misleading associations of an unfortunate phrase.

To return to the real, from the seeming, import of the phrase, we see that, as the offer of land is in general attended with no real cost, a tax upon the payment for land does not disturb production.[1] On grounds of distribution, too, a sort of income which increases without any effort on the part of the recipient is *prima facie* a suitable object for a specially heavy impost. On these grounds Mill's proposal to tax away the future unearned increment of rent is defensible, if accompanied with Mill's proviso, that existing interests should not be disturbed. For, as argued elsewhere,[2] a special tax on existing incomes from land would violate the two principal conditions of a good tax: it would both tend to diminish the amount of production, and also to impair the equality in the distribution of burdens between the owners of incomes derived from land and from other kinds of property.

The practical importance of Mill's proposal is greatly reduced by the proviso with which it is accompanied. For, in order that the State may make a good bargain by giving the market price for a certain class of future goods, the State must be able to look further ahead—must exercise the telescopic faculty of prospectiveness in a higher degree—than the ordinary capitalist. And it may well be doubted whether this condition is fulfilled by the politicians who act on behalf of the State. We hear much of instances, like that of Chicago, where the value of sites

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1891; Wicksteed, *Laws of Distribution*, p. 47 (the last critic not referring *nominatim* to Professor Marshall). For a more sympathetic criticism of Professor Marshall's doctrine see ECONOMIC JOURNAL, Vol. V. p. 589.

[1] As Professor Carver said lately (at the banquet of the Massachusetts Single Tax League, 1902), a person who thinks that the repressive effect of a tax on land is at all comparable with the repressive effect of a tax on the products of industry must have an eye for exceptions like "a certain senator of whom it was said that he could see a fly on a barn-door without being able to see the barn or the door either." The incident in question may be elucidated by representing the "supply-curve" of land as a perpendicular line. *Cp.* II. 69.

[2] II. 198 *et seq.*

is said to have multiplied some eighty-fold in half a century; but we hear little of proposals to buy up at their present market value the site of some future Chicago, unless, indeed, as part of a scheme for Land Nationalisation, which does not include compensation to vested interests. Unlike the husbandman, who plants trees the fruit of which he will not himself see, the advocates of a single tax and other socialist agitators grasp at the standing crop which has been sown by others, heedless whether cultivation in the future is thereby discouraged.

But, even if their outlook were as distant as it is bounded, there would remain the possibility that, though looking far ahead, they might not discern distant objects clearly. Mill cannot be accused of the shortsightedness which sacrifices the future to the present. He looked very far ahead. But he did not see what was coming, the fall of English rents. Actuated by the highest motives, he proposed an arrangement which was perfectly just to the landlords, and would have proved perfectly disastrous to the State.

Passing in the traditional order from *Land* to *Labour*, we may begin by considering a very abstract labour market, in which the difficulty caused by the "advance" of wages is kept out of sight.[1] The following example of such a labour market may be worth reproducing, although it is not a genuine case of Distribution :—

Let us suppose several rich men about to ascend some an easy mountain, some a difficult one, each ascent occupying a day. And let these rich travellers enter into negotiations with a set of porters who may be supposed many times more numerous than the employers. An arrangement according to which the remuneration for ascending the easy and the difficult mountains was the same could not stand : it would not be renewed from time to time. For some of the porters employed on the difficult mountains, seeking to minimise the disutility of their task, would offer their services to travellers on the easy mountains at a rate somewhat less than the temporarily prevailing one. Nor would equilibrium be reached until each porter employed on a difficult mountain received an excess above the fee for the ascent of an easy one sufficient to compensate him for the extra toil. At the same time—simultaneously, in a mathematical

[1] There is an abstract point of view from which, as Professor Barone well observes (*Giornale degli Economisti,* loc. cit.), the circumstance that wages are paid in advance is of secondary importance.

sense—the increment of satisfaction due to the last porter taken on by each traveller would just compensate the purchaser of that labour for his outlay on it.[1]

In this example the great number of the employees as compared with the employers is not an accidental circumstance. Suppose that the arrangement which is common in the Tyrol—that each amateur ascensionist should be accompanied by only one guide—were for technical reasons universal. Then the bargain between travellers, on the one hand, and guides, on the other, would not in general be perfectly determinate. It would still indeed be true that "an arrangement according to which the remuneration for ascending the easy and the difficult mountains was the same could not stand." But it would no longer be true that the remuneration for the easy mountain—or, rather, for the average mountain, from which the fares both of the easier and the more difficult ascents might be measured—would be in general determinate.[2] There would in general exist no force of competition by which any particular arrangement (as to the average mountain) initiated by custom and accident could be disturbed. That is, still supposing the service of a guide or porter to be sold as a whole. For, if the labour of the assistants can be sold by the hour, or other sort of differential dose, the phenomenon of determinate equilibrium will reappear. There seems no reason to think that the case of indeterminate equilibrium which has been illustrated is other than exceptional in the actual labour market, even where the bargain appears to be made for totals as distinguished from doses of labour,—situations rather than tasks. For there is, in fact, such a variety of situations attended with different amounts of work[3] as probably in practice to realise that divisibility of the thing supplied—here labour—which, together with the divisibility of the thing demanded,—here money,—constitutes a condition of a perfect market with determinate equilibrium.[4] * Still, the

[1] ECONOMIC JOURNAL, Vol. IV. p. 225.

[2] As argued in *Mathematical Psychics*, p. 42.

[3] *Cp.* Marshall, *Principles of Economics*, Book VI, chap. ii. § 2, note, p. 599, 4th edition. Consider the case of managers, above, p. 180.

[4] *Mathematical Psychics*, p. 18.

* Though one condition of a perfect market is thus secured, it does not follow that the labour-market will be perfect. Let us start with any system of bargains between entrepreneurs and work-people (presumed not to be capable of serving two masters at the same time). Then, there being supposed a variety of situations and tasks, let the round men in square berths change places with the square men in round berths with advantage to all (entrepreneurs included). There will thus be reached a settlement such that it cannot

point of theory is worth notice. Perhaps the friction in the labour market would be less if labour were sold freely by the hour (or other small "dose").

It ought to be mentioned that a different view of Exchange has been taken by a high authority on Distribution. Professor Böhm-Bawerk presents as the general type of a market that very case which is here regarded as exceptional. On one side of the markets are put dealers each with a horse—or it may be a batch of several horses [1]—which he will not sell under a certain price, on the other side buyers each of which will not go beyond a certain price. The following scheme is given as an example of such data : [2]—

| *Buyers.* | | *Sellers.* | |
|---|---|---|---|
| $A_1$ values a horse at . . . (and will buy at any price under). | £30 | $B_1$ values a horse at . . (and will sell at any price over). | £10 |
| $A_2$ ,, ,, ,, ,, | 28 | $B_2$ ,, ,, ,, ,, | 11 |
| $A_3$ ,, ,, ,, ,, | 26 | $B_3$ ,, ,, ,, ,, | 15 |
| $A_4$ ,, ,, ,, ,, | 24 | $B_4$ ,, ,, ,, ,, | 17 |
| $A_5$ ,, ,, ,, ,, | 22 | $B_5$ ,, ,, ,, ,, | 20 |
| $A_6$ ,, ,, ,, ,, | 21 | $B_6$ ,, ,, ,, ,, | 21 10*s.* |
| $A_7$ ,, ,, ,, ,, | 20 | $B_7$ ,, ,, ,, ,, | 25 |
| $A_8$ ,, ,, ,, ,, | 18 | $B_8$ ,, ,, ,, ,, | 26 |
| $A_9$ ,, ,, ,, ,, | 17 | | |
| $A_{10}$ ,, ,, ,, ,, | 15 | | |

From these data it is deduced that the price of a horse must be between £21 and 21 10*s.* But, if the data had been different,

be disturbed with advantage to each and all; except by the employers competing with each other for workmen. Suppose the settlement to be such and so favourable to the work-people that it cannot be disturbed by the competition of the employers; then, the market will be indeterminate, just as if the work-people were all equally efficient. Accordingly, "There is no determinate and very generally *unique* arrangement towards which the system tends under the operation of, may we say, a law of Nature, and which would be predictable if we knew beforehand the real requirements of each, or of the average, dealer; but there are an indefinite number a priori possible settlements (see B. II. 313 and references there given).

[1] In the criticism of the *Positive Theory of Capital*, at p. 333 of the ECONOMIC JOURNAL, Vol. II., repeated from the Address to the British Association, Section F, 1889 (reprinted in the *Journal of the Statistical Society*, December, 1889), it was too leniently suggested that the author, in a subsequent note (p. 214, Smart's translation of *Positive Theory*), brought in the essential circumstance which his main illustration omits; namely, doses with varying marginal utility. It would rather seem, however, that the stud of horses permitted in the said note does not differ essentially from the single horse of the main illustration. It seems to be treated as a mass of commodity which the seller offers, the buyer takes or leaves, as a whole. At any rate, the writer has failed to see the significance of divisibility in the commodity. For, otherwise, he would not have attributed so much "latitude" (*loc. cit.* quoted in the text) to the case in which the sellers (and likewise the buyers) do not differ from each other in their subjective valuation of a horse.

[2] *Positive Theory of Capital* (translated), Book IV. chap. iv.

the price might not have been thus determinate. "If there are, for instance, ten buyers who each value the commodity at £10, and ten sellers who each value it subjectively at £1, obviously all the ten pair can come to terms, and the zone which lies between the valuation of the last buyer and the last seller represents the wide latitude between £1 and £10." Of this character, according to the writer, are the circumstances of the labour market.[1] In such a case some further datum is required to determine price. "That this latitude should be narrowed down, the further circumstance must be present that the desire of the buyers is directed to an unlimited number of goods, while at the same time the total amount of means of purchase must be strictly limited, and the buyers must be determined to spend the whole of this sum in purchase of the commodities in question."[2] This condition is fulfilled, according to Professor Böhm-Bawerk, by the "general subsistence market."

This example will hardly be accepted as typical of a market by the mathematical economists who walk in the way of Gossen. Agreeing with the Austrian leader that value rests at bottom on subjective estimates, they will accept his scheme, just as they would accept the description of a common auction, as illustrative of that attribute. But they may complain that the illustration does not illustrate another attribute which they regard as essential to the determination of value in a market,—the circumstance that each party on the one side is free, in concert with some party or parties on the other side, to vary the amounts of those quantities on which depends his advantage—the *quid* and the *pro quo*—up to a limiting point, or *margin* at which he estimates his advantage to be a maximum. The "marginal pair" of the Austrian scheme hardly exemplifies the *law of marginal utility*. We require to know, not so much the least price which each horse dealer will take for his horse or stud,[3] but how much horseflesh each individual, or at least all collectively, will offer at each of several prices, with similarly graduated data for the would-be buyers. Granted data of this sort, the mathematical economist need not trouble himself much about a matter which is vital according to the Austrian scheme,—whether the "subjective valuation" of a horse is the same (or very similar) for all the sellers, while the dispositions of the buyers are likewise identical. The case of like dispositions does not constitute a special variety

[1] *Op. cit.*, Book IV. chap. v. p. 217; Book VI. chap. v. ("On the General Subsistence Market").

[2] *Loc. cit.*

[3] See note 1, p. 37.

of the problem, one which is insoluble without additional data. Far from being anomalous, that case may be normally assumed as a harmless and convenient simplification, very proper to an introductory statement of the general theory.[1]

> "Nec Deus intersit, nisi dignus vindice nodus
> Inciderit"—

The case of like dispositions does not present any peculiar difficulty calling for so very mechanical a *Deus ex machinâ* as the hypothesis that "the total amount of means of purchase must be strictly limited and the buyers must be determined to spend the whole of this sum in purchase of the commodities in question." It is riding a one-horse illustration to death to put the accidents of an exceptional sort of auction as representative of the actual transactions by which the great mass of national income is distributed.

This criticism, it must be freely admitted, involves an issue about which legitimate differences of opinion may exist,—what is the most appropriate conception of the process by which value is determined through the higgling of the market? Any simple conception must involve a considerable element of hypothesis, not admitting of decisive proof. The hypothetical character of the inquiry will appear if we look back to that model labour market in which guides or porters were supposed to be hired by amateur mountaineers. It was tacitly assumed that each party has certain dispositions as to the amount of money that he is willing to give or take in exchange for a certain amount of work,—a scale of subjective estimates[2] which is supposed to be formed before the parties come into communication, and not to be modified by the chaffering of the market. The constancy of these dispositions being assumed, it is presumed that somehow a state of equilibrium will be brought about, such that the party on one side cannot improve his position by entering into new contracts with some party or parties on the other side. The better opinion is that only the position of equilibrium is knowable, not the path by which equilibrium is reached. As Jevons says, "It is a far more easy task to lay down the conditions under which trade is completed and interchange ceases than to attempt to ascertain at what rate trade

[1] It is so assumed in *Mathematical Psychics*.

[2] Whether expressed by a demand-curve (or schedule, *cf.* Marshall, *Principles*, Book III.) or by way of *indifference curves*, as Professor Pareto has suggested (*Giornale degli Economisti*, 1900).

will go on when equilibrium is not attained."[1] Particular paths may be indicated by way of illustration, "to fix the ideas," as mathematicians say.[2]

In this spirit two kinds of higgling may be distinguished as appropriate respectively to short and long periods. *First*, we may suppose the intending buyers and sellers to remain in communication without actually making exchanges, each trying to get at the dispositions of the others, and estimating his chances of making a better bargain than one that has been provisionally contemplated. By this preliminary tentative process a system of bargains complying with the condition of equilibrium is, as it were, rehearsed before it is actually performed. Or, *second*, one may suppose a performance to take place before such rehearsal is completed. On the first day in our example a set of hirings are made which prove not to be in accordance with the dispositions of the parties. These contracts terminating with the day, the parties encounter each other the following day,[3] with dispositions the same as on the first day,—like combatants *armis animisque refecti*,[4]—in all respects as they were at the beginning of the first encounter, except that they have obtained by experience the knowledge that the system of bargains entered into on the first occasion does not fit the real dispositions of the parties. The second plan of higgling was supposed in the example,[5]—the plan which is more appropriate to "normal" value.

Contemplating the theory of exchange in the abstract, we may exclaim with Burke, "Nobody, I believe, has observed with any reflection what market is without being astonished at the truth, the correctness, the celerity, the general equity, with which the balance of wants is settled."[6] But, when we come to the labour market, or any particular market, we must carefully inquire with what degree of approximateness the above-stated fundamental postulate[7] holds good. When the bargaining

[1] *Theory*, 2nd edition, pp. 101–2. The context seems to impose an unnecessary limitation: "Holders of commodities will be regarded not as continuously passing on these commodities in streams of trade, but as possessing certain fixed amounts which they exchange until they come to equilibrium." The "fixed amount" may be considered as renewed from time to time for each of the individuals placed along a "stream of trade" (see below, p. 197).

[2] This view of the subject is presented at greater length in an article in the *Revue d'Économie Politique*, January, 1891. [See note appended to α (Section VI)].

[3] They *recontract*, in the phraseology of *Mathematical Psychics*.

[4] *Æneid*. xii. 788.

[5] Above, p. 35.

[6] *Thought and Details on Scarcity*. He is speaking with special reference to the labour market.

[7] Above, p. 39.

extends over a considerable time, changes are apt to occur in the dispositions of the parties, whether independently of each other and sporadically, or in a manner even more fatal to the theory, by way of imitation.[1] Also, where there occurs a series of encounters between buyers and sellers, the results of the earlier encounter may affect the dispositions with which the later ones are entered on. The terms which the labourer is ready to offer and accept are altered by the alteration in his habits and efficiency which is the consequence of previous bad bargains.[2]

The peculiarities of the labour market pointed out by Professor Marshall go far to modify the general presumption in favour of *laisser faire*. But less careful writers are less successful in supporting the burden of proof which lies on those who profess to add to or take away from that outlined theory of Exchange which seems to express all that is known *in general* about the working of a market. A warning example of such modification not warranted by specific experience is the doctrine of the wage-fund, which is now universally discredited, and ought always to have excited suspicion and challenged proof because, as already intimated in another connection, it is a supposition repugnant to the general theory of Exchange that "the total amount of means of purchase must be strictly limited, and the buyers must be determined to spend the whole of this sum in purchase of the commodities in question."[3] Perhaps, as Sir Leslie Stephen says with reference to the classical writers, "the assumption slipped into their reasoning unawares."[4] Sometimes it may have been intended only to convey that early lesson which is contained in our opening paragraphs,—that no party to production can expect to earn more than the total produce. Sometimes there was contemplated a more definite statement true of short periods,—a truth which has been well stated by Professor Taussig in his article on "The Employer's Place in Distribution," and at greater length in his book on *Wages and Capital*—

"The whole of the real income available for the community is not in any substantial sense at the disposal of the capitalists. . . .

[1] See Pigou on "Utility" in the ECONOMIC JOURNAL for March, 1901. Compare, as to the absence of predeterminateness in the dispositions of parties to the labour market, Walker, *Political Economy*, Art. 320.

[2] *Cp.* Marshall, *Principles of Economics*, Book VI. chap. iv., and Walker, *Political Economy*, Art. 308 *et seq.*

[3] Quoted from Böhm-Bawerk, who himself compares his theory with that of the wage-fund (*Positive Theory*, p. 419). Both theories seem true of short periods. The context accords with the view here taken of the theory, as true of short periods, inadequate to long periods.

[4] *The English Utilitarians*, Vol. III. p. 216.

A large part of the commodities now on hand would not serve their turn. The supply of bread and flour and grain at any moment is adjusted to the expected needs of the whole mass of consumers. . . . The effective choice which the capitalists would have . . . would be thus confined, for the time being at least, within limits not very elastic."[1]

Let us suppose that the working classes live on bread only, while the capitalist classes consume buns also. On a day, after a conference between employers and employed, the partition of the national dividend is altered in favour of the capitalists. Yet they will be unable to benefit immediately by the change. On that day more buns will not be forthcoming, all the bakers' ovens being preoccupied with bread.

For the purpose of illustration there has been chosen a specially simple case in which the articles consumed by the two classes are formed out of the same material, and by a process which is identical up to the penultimate stage. The stream of production does not bifurcate till it debouches into the mouths of the two parties to Distribution.

When we consider longer tracts of that stream, there comes into view a circumstance to be discussed under the head of *Capital*, the influence of time on value. To illustrate the distribution of produce between those who have contributed at different times to its production, let us at first make abstraction of other differences, and imagine economic men uniting the functions of workman and capitalist-entrepreneur, differing only in the amount of capitalisation, the length of time during which their labour is invested. One labours at proximate means, another at remote means, tending to the ultimate product out of which all the producers are remunerated. An idea of a train of production formed by successive operations directed to an ultimate product may be obtained by watching any factory. Here you have the raw cotton-wool put in, there you see a "sliver" of carded cotton flowing from one machine *en route* to another, until at the last stage there comes out the finished article. To illustrate the process of distribution, we must now conceive a backward flow of the ultimate product to the several producers. We might imagine each one's share to be conveyed to him by some contrivance like those wondrous little vehicles in the Boston Public Library, which, as if gifted with human intelligence, find their way about the building to the particular

[1] *Quarterly Journal of Economics*, Vol. X. p. 74.

place where each book belongs. To illustrate the effect of distance in time on distribution, we must further modify the model presented by an ordinary factory. We must suppose the interval of time between the processes to be greatly magnified, months being substituted for minutes. Then there will come into view the circumstance to which attention is particularly directed,—that a larger share will be conveyed to each producer (other things being equal), the greater his distance from the final stage. There will thus be a continual flow of materials in process of manufacture onwards and of products ready for consumption backwards, if the work at each stage is steadily maintained,—provided that there is a continual stream of raw material, and that the machines are continually renewed.[1] Considering the continuous round of production and consumption, we realise the important truth which Mill has thus expressed :—

"The miller, the reaper, the ploughman, the plough-maker, the wagoner and wagon-maker, and the sailor and ship-builder, when employed, derive their remuneration from the ultimate product,—the bread made from the corn on which they have severally operated or supplied the instruments for operating." [2]

To represent the continual expansion of value as the present ripens into the future, a series of concentric circles has been happily employed by Professor Böhm-Bawerk.[3] Varying his illustration, let us suppose the circles to be drawn on ground which rises uniformly from the outmost circle towards the centre O in the accompanying diagram at which the apex tapers to a needle-point.[4] The circles are drawn at equal distances as measured on the surface, and therefore, in a bird's-eye view which the diagram is intended to represent, become huddled together in the neighbourhood of the central height. Across the circles, down the hill, flow streams with uniform velocity, so as to pass from circle to circle in a unit of time. The breadth of a stream increases with its length,—not in direct proportion to the length, but according to the law of *accumulated* price.[5] The volume of the stream is proportioned to its breadth and to its depth (not shown on the figure). The stream takes its rise at some position on the channel (*e. g.* at $a_5a'_5$), the flow per unit of time at that point being proportioned to the energy put forth in pumping

[1] *Cp.* p. 46, below.

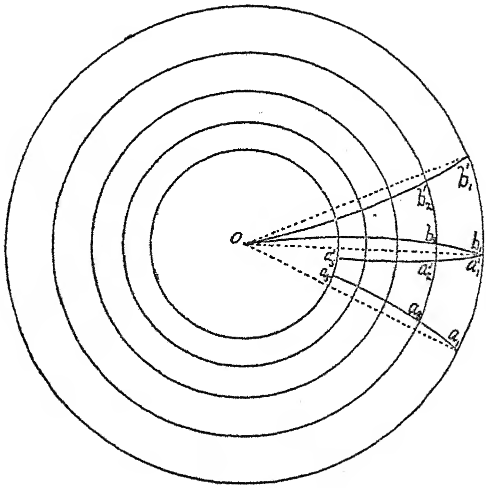
[2] *Political Economy*, Book I. chap. ii. §§ 1, 2.

[3] *Positive Theory*, Book II. chap. v.

[4] The series of highering circles is not shown in the diagram after the fifth circle.

[5] Marshall, as cited above, p. 19, note 2.

from a certain source. As the volume thus originated rolls down the channel, it continually increases by infiltration from the neighbouring soil without any additional pumping, so that, the depth being preserved constant, the volume is proportioned to the increasing breadth.[1] Besides this increase due to its defluxion, the volume may also in the course of its downward flow be increased by additional pumping from a second source (e. g. $a_2a'_2$). This second increase corresponds to an increase in depth (not shown in the figure); and this second contribution is augmented, like the first, by the infiltration which attends



defluxion. There may be as many sources as there are circles cut by the descending stream. But there need not be a source at each interval. The equidistant circles correspond to successive lines, not always coincident with successive stages of production at each of which additional labour is applied.[2] The train of

[1] The broadening of the stream corresponds to the two consilient facts, that future pleasures are discounted and that production is increased by "roundabout" methods. As to the first of these facts, see in Marshall's *Principles of Economics* the passages which relate to *discounting future pleasures*, and the remarks on those passages in the review of the second edition of the *Principles* in the ECONOMIC JOURNAL, Vol. I. (1891) p. 613. See also the admirably clear explanation and illustration given by Professor Carver in his article on "Abstinence and the Theory of Interest," *Quarterly Journal of Economics*, Vol. VIII. (1893) p. 48. As to both the first and second facts, see Böhm-Bawerk's well-known expositions. But as to the consilience of the two facts see, rather, Professor Marshall on the "fundamental symmetry" between the action of Supply and Demand (noticed in the review referred to). See also Professor Carver's explanation of the double statement that interest is payment for the sacrifice of abstinence, and that interest is paid because capital is productive (*loc. cit.* p. 43).

[2] Corresponding to the machines in the illustration given in the preceding paragraphs.

production thus represented terminates in a product ready for consumption—it may be loaves or ribbons, wine or shoes—on the shore of a circumfluent sea of commodities. As in the natural world rivers are replenished by the melting of the snow, which is formed on mountains by the congelation of vapour, which is wafted up from the ocean, into which the rivers flow down, so in the *mundus economicus*, by a compensation carried into more just detail, labour is restored and re-created by a refreshing rain of commodities derived from that sea into which all finished commodities are discharged. Volatile shoes and wine, and other commodities in due admixture up to a certain value, find their way to each point upon the heights from which a source has been tapped, the volume of this return corresponding to the volume of the original contribution,—not indeed the same, but the same increased by a factor of accumulation, the ratio which the breadth of the stream at the littoral bears to its breadth at the point of origin (e. g. $a_1a'_1 : a_5a'_5$). The flight of the commodities from the littoral to the heights need not be supposed to occupy an appreciable time.

The idea of a Flow which has been illustrated is primarily applicable to the case in which materials and consumable commodities are used up once for all within a unit of time. But the case of labour invested for longer periods is easily assimilated. Suppose that a plough lasts five years, and that in each year of its existence it makes an equal addition to the consumable crop, the year being taken as the unit of time. Then, although the plough may have been made in a week or month, the labour of its production is to be considered as invested in five unequal portions at unequal distances in time from the epoch at which the invested labour meets with its return. The total labour of making the plough may be considered as applied at several positions ($a'_1a_1$, $a_2a'_2$, . . . $a_5a'_5$) in several contributions, respectively proportioned to the breadth of the stream at these points. If labour is invested in the production of a machine, imagined by economists, which lasts for ever,[1] or, what comes to the same, an improvement, such as the draining of land or opening a mine, or cutting an isthmus, which is calculated to yield a constant income for an indefinitely long series of years, then the series of positions along the stream at which the labour is supposed to be invested must be carried back indefinitely (see the channel of which the mouth is $b_1b'_1$) up to that needle-point whose tapering dimensions correspond to the perspective of an indefinitely distant future.

[1] Mill, *Political Economy*, Book I. chap. vi. § 2.

Eternal machines are not very common; but the conception may serve to illustrate a species of tool or implement of which the race remains immortal, though the individual is worn out and perishes. Of this kind are implements which are directed not only to produce goods immediately ready for consumption or implements of a kind different from their own, but also to reproduce their own kind. Hammers and axes are presumably of this kind in a primitive society; in an advanced state of industry, some more complicated engines.[1] Such machines may be compared to horses, if used not only as beasts of burden, but also as stallions. The demand for such creatures is presumably influenced by the expected series of future generations, so far as commercial prospectiveness may extend. In the stationary state of steady motion, here provisionally contemplated, reproductive machines would be illustrated by beasts of burden of which the breed does not sensibly improve in successive generations.

Two channels only have been represented in the diagram, one of finite, the other of infinite length, with breadth exaggerated for the sake of clearness. Properly, there should be as many channels as there are categories of articles ready for immediate consumption,—"goods of the first order," as the Austrians say; and the breadth should be such as to allow of the corresponding number of sectors being fitted into the circle. Another circumstance which must be left to the imagination is the introduction of one and the same article into several streams of production at different distances from the final stage. Coal, for instance, so far as it is used for warming dwelling-houses, is a good of the first order; so far as it is used to drive machines,—themselves perhaps used only to produce other machines,—coal is to be placed among the higher orders.

The distinction which has been drawn between work which is applied in the neighbourhood of and at a distance from the final stage of production is not coincident with the distinction between the saving and the non-saving classes. The shower of commodities apportioned to each spot according to its height above the littoral as well as to the volume of value which there took its rise, is not "like the gentle rain from heaven." It does not drop impartially on all who have been concerned with the work of eliciting the stream. Those who have done the common labour of pumping—the drawers of water—fare no better than if that work had been done at the littoral. In fact, it is proper to

[1] Or rather a certain system of machinery. *Cp.* Marx on machines produced by machinery. *Capital*, ch. xv.

conceive that it was done at the littoral. As the energy generated at the Falls of Niagara is transmitted for use to a point higher up on the river, so on the stream of production the work of pumping is mostly done at the littoral, though it is applied at the heights. For instance, on the first stream an amount of work proportioned to $a_5a'_5$ might be done at the littoral, and be paid for in commodities at the rate current on the littoral; that is, without the augmentation of value which is due to defluxion. The remainder of the volume of value which is discharged per unit of time flies off to those who occupy the height represented by $a_5a'_5$.

If now it is asked where *rent* comes into this representation of distribution, the answer is to be found in the theory (above, p. 33) that from the point of view of the entrepreneur the use of land appears in the same light as the use of labourers,—as a factor of production. The idea of a steady cyclic flow which we are striving to win becomes not much more complicated when we imagine that those who, placed on the heights, preside over the origination of productive streams, obtain the material that is to form the current, the precious fluid which it is their office to start upon its downward flow, not solely from a pumping proletariat, but also from the fortunate owners of springs which gush spontaneously. There is, indeed, this difference between the labourer and the land-owner: that, whereas the former (even in the present age and still more when the classical economists flourished) has to spend a great proportion of his daily wage upon his daily necessaries, and therefore in respect of the bulk of his income *must* be placed at the littoral line, the latter *may* save a great part of his income, when it is greatly in excess of his daily necessaries, and in particular, with respect to that great portion, may defer fruition until the stream shall have flowed down from the point at which his contribution is applied to the point at which production becomes merged in consummation. Another difference between land and labour in their relation to capital and enterprise arises from the circumstance that, unlike the labourer (in a free country), land itself, as well as its use, is sold. Whence arises a well-known correspondence between rent and interest in their relation to the capital value of land. This similarity will not be mistaken for identity [1] by those who find the essential

[1] "The attempt of certain writers to refine away this traditional distinction between land and capital, rent and interest, impresses me as a subtle obscuration of plain facts," well remarked one of the speakers at the recent banquet of the Massachusetts Single Tax League (1902).

attribute of rent in the *limitation* of the objects for which rent is paid.[1]

To complete the analysis of the parties to Distribution, it may next be required to distinguish the capitalist from the entrepreneur. They are both easily distinguished from the salaried manager in that he is at the littoral, in that respect like the common workman, while they are both above that line. But to draw a line in the series of shades which intervene between the employer of Walker's type and the mere shareholder, to determine at what point the capitalist ends and the entrepreneur begins, appears to defy analysis. As Thought and Emotion are inseparably blended, though one may so far preponderate as to give its name to the state of consciousness at any time, such is the inseparable connection, such the intelligible but not exactly definable distinction, between Enterprise and Saving. The indefiniteness of the relation is illustrated by the shifting use in economic literature of the term Profit.[2]

That profit other than remuneration for managerial work should be transmitted to those who occupy a position on the heights—often the easy position of a dormant shareholder—is certainly invidious and difficult to justify to those who toil below. Yet it may be reflected that the condition of those below would have been worse if those above, or those from whom they purchased or inherited their position, had not been content to wait for future goods instead of grasping at immediate pleasure. The Flow so beneficial to all classes would never have been set up without abstinence.[3] It could not continue in its present magnitude but for the continued abstinence of each one who has a right to dispose of wealth which is in course of production,—make a bonfire of it, if he can get a momentary pleasure from that extravagance, or by some less simple, though more familiar increase of unproductive consumption "eat up his capital."

The consequences of an increase in unproductive consumption may be contemplated by reversing the consequences of an

[1] *Cp.* above, p. 32, Marshall, *Principles, sub voce* "Rent."

[2] As instructively pointed out by Mr. L. L. Price in his article on "Profit-sharing" published in the ECONOMIC JOURNAL, Vol. II. (1892), and in his *Economic Science and Practice*, p. 75 and *ante.*

[3] Compare Adam Smith. "By what a frugal man annually saves he not only affords maintenance for an additional number of productive hands for that or the ensuing year, but, like the founder of a workhouse, he establishes, as it were, a perpetual fund for the maintenance of an equal number in all times to come." *Wealth of Nations*, Book II. chap. iii. In our metaphor, taking up a new position on the heights corresponds to this establishment of a perpetual fund.

increase in parsimony. The latter increase forms part of a larger subject, economic progress. The progressive change in the volume of value and channels of production cannot be understood until there has been attained what was the object of the preceding paragraphs,—the clear idea of a steady flow in channels for a time unchanged.[1] The study of this stationary state is perhaps the part of economic science which principally deserves to be described as theory of Distribution. In these pages it is not attempted to go far beyond the comparatively narrow round of steady motion in fixed cycles of production and consumption. It must suffice to indicate three species of progressive alteration in the economic mechanism. There is, *first*, a uniform increase in the number of both capitalists and labourers, or, more generally, capital and labour, other things being the same. This change presents no difficulty: it may be represented by an increase in the *depth* of all the channels. *Second*, the rate at which the breadth of the channels diminishes as one ascends from the littoral—in other words, the rate of interest—might be diminished. A limiting case of this species is put by Mill when he supposes unproductive expenditure of capitalists to be "reduced to its lowest limit." Conceivably, this change might have no other effect than to reduce the portions accruing to the capitalists—such as $a_1a'_1$—$a_2a'_2$—to a minimum. The capitalists with new eagerness bid against each other for the service of the labourers; but, if the latter do not give more work for higher pay, the consequences might be a new equilibrium in which the same volume of value is steadily rolled down the same channels of trade, though the portion which flies back to the heights is a minimum. But, even if the quantity of value continued constant, it is hardly to be supposed that the quality [2] of the commodities which make up the amount would remain unchanged. And, in fact, an increase of wages would probably be followed by an increase in the number and efficiency of the wage-earning classes.[3] And these results would favour the occurrence of a *third* kind of progress which may, however, be considered as arising independently of the others; namely, the lengthening of the trains of production.[4]

[1] On the nature of the steady flow with which we are concerned see Marshall, ECONOMIC JOURNAL, Vol. VIII. p. 40, and *Principles of Economics, sub voce* "Stationary State."

[2] *Cp.* Mill, *loc. cit.*,—"there would no longer be any demand for luxuries on the part of capitalists."

[3] *Cp.* Marshall, *Principles*, Book IV. ch. xiii.

[4] It is possible, as Mill shows, *Political Economy*, Book I. chap. vi. § 2 (*cp.* Ricardo on machinery and Mr. Pierson, *Principles of Economics*, p. 311), that lengthening the period of investment, and also invention, while it increases

It may be doubted whether any great lengthening of the trains is possible without a concomitant improvement in the arts of production; yet, as Sidgwick observes,[1] invention is not necessarily followed by increase of capitalisation.[2]

The third head of progress even more surely than the second will be attended with changes in the channels of production. As already observed [3] with reference to the portion of truth contained in the wage-fund theory, time will in general be required for the carrying out of such changes. The means of production which are rolling down the channels at the instant when the change begins must all or in great part be suffered to run out: otherwise there will probably be a considerable waste of labour, and interruption to consumption. One delicate adjustment which would be deranged can only be alluded to here—the monetary circulation, especially that form of it which consists of debts that are continually "cleared," or cancelled. We might imagine the flow of factors in the channels of production and the flight of finished products backward on the way to consumption to be attended each with a displacement of air in a direction opposite to the main movement,—light counter-currents which have their use in facilitating the movements of solid wealth, and in the fulfilment of their useful function continually meet and neutralise each other. But, evidently, we have reached the degree of complexity at which the illustration becomes more difficult to understand than the thing which is to be illustrated. For a more concrete embodiment of a more complete theory the student is referred to the *Principles of Economics*,—a reference of which the value is, if possible, enhanced by the solid work which Mr. N. G. Pierson has published under the same title.[4]

The preceding hints and metaphors and warnings may assist the student to obtain a general idea of the process by which distribution of the national income is effected. An outline of theory so abstract is not to be despised as useless. It satisfies a legitimate curiosity. It is part of a liberal education. It is comparable in these respects with an elementary knowledge of

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the amount of goods accruing to the capitalist, may diminish the amount accruing to the workers. What Mill says in this connection of the "fresh creation" of capital and "additional saving consequent on improvements" is made more intelligible by the use of the illustration here offered.

[1] *Political Economy*, Book I. chap. iv. § 8.

[2] *Loc. cit.* Mill treats capital and arts of production as independent variables. *Political Economy*, Book IV. chap. iii.

[3] Above, p. 41.

[4] Translated into English from the Dutch by Wotzel.

astronomy. Such knowledge will not be of much use in navigation. And yet it has a certain bearing on real life. The diffusion of just notions about astronomy has rendered it impossible for astrologers any longer to practise on the credulity of mankind. A knowledge of first principles affords a test by which the authority of those who offer themselves as guides may be estimated. A little science has a further use: it is of assistance in obtaining more.

As the astronomer will proceed from a first approximation to a second, so economists should soften the hard outline of abstract theory by a regard to particular circumstances. As he in dealing with a new object will make certain of his first approximation,—will consider, for example, whether an ellipse or a parabola fits better to the orbit of a new comet,—so it behoves us to consider whether the classical hypothesis presupposed in the preceding pages [1]—two-sided competition [2]—is appropriate to the conditions of modern industry. The hypothesis of two-sided monopoly [2] is strongly suggested by what we see before us,—consolidated capital confronted by consolidated trade unions. But it is alleged that beneath that appearance the forces of competition are effectively at work; that the settlement which is apt to be, and ought to be, agreed to between a combination of Capital and a combination of Labour is no other than that which would have been determined by competition if the individuals now combined had been free to act competitively. No one has expressed this view with more authority and decision than Walker :—

"Competition, perfect competition, affords the ideal condition for the distribution of wealth." [3]

"Competition affords the only absolute security possible for the equitable and beneficial distribution of the products of industry." [4]

To the same effect, Professor Clark, when he teaches that—

"The question whether the labourer is exploited and robbed depends on the question whether he gets his product." [5]

What is meant by getting his product appears from the following passages :—

"What we are able to produce by means of labour is determined by what a final unit of mere labour can add to the product that can be created without its aid." [6]

[1] See the opening paragraphs above, p. 14.
[2] The useful phrase of Dr. Böhm-Bawerk.
[3] *Political Economy*, par. 466. [4] *Ibid.*, par. 467. *Cp.* par. 343 *et seq.*
[5] *The Distribution of Wealth*, chap. i. [6] *Ibid.*, p. 180.

"If each productive function is paid for according to the amount of its product [thus reckoned], then each man gets what he himself produces."

The ideal of just arbitration is that—

"Men should get something approximating the part of that joint product which they may fairly regard as solely the fruit of their own labour.[1] The basis of the claim that a workman makes is that his presence in a mill causes a certain increase in the output of it."[2]

If these views are generally accepted, the analysis of bargains in a regime of competition will retain its importance. But it may well be doubted whether these views will be generally accepted, even by the thoughtful few, much less by the more numerous of the concerned parties. First, it may be objected that the same principle will give very different results according to the relative numbers of the parties. Put a case which has actually existed, or at least may be well supposed to have existed, in order to test the general application of the principle,—the case in which the number of the employees is not much greater than, say not more than twice as great as, the number of the employers. In such a case, if labour is sold by the hour,—openly, or virtually in a fashion that probably prevails at present,[3]—there would be a determinate equilibrium of the labour market such that each labourer would earn an amount equal to the number of hours worked, multiplied by the final productivity of each hour. That arrangement might appear just, on a certain interpretation of the dictum that one's product "is determined by what a final unit of mere labour can add to the product." But the arrangement would not be just if "the basis of the claim that a workman makes is that his presence in the mill will cause a certain increase in the output of it." All turns on the unit employed. If it is allowable to take the hour as the unit, and find the wage of the individual man by multiplying the number of hours worked by the final productivity of the unit, why should it not be allowable to take a *gang* of men as the unit, and find the wage of the individual man by dividing the number of men in a gang into the final productivity of a gang? Not to rest the argument on supposed cases, take the case of the "capitalist" as he existed in Ricardo's time, or even the modern entrepreneur who is not a salaried

[1] "Authoritative Arbitration," *Political Science Quarterly*, December, 1892, p. 559.

[2] *Ibid.*, p. 559.

[3] See Marshall, *Principles of Economics*, Book VI, chap. ii. § 2, note to p. 499, 4th edition, referred to above.

manager. If such a one is to be paid on the basis that "his presence in a mill causes a certain increase in the output of it," it is quite possible that he would be justified in claiming a much larger share of the joint product than he now obtains.[1] The assertion that the entrepreneur receives just as much as he adds to product is at best an empirical law,[2] not possessing the sort of universality proper to a general canon of distributive justice. Thus the coincidence of perfect competition with ideal justice is by no means evident to the impartial spectator: much less is it likely to be accepted by the majority of those concerned, whose views must be taken into account by those who would form a theory that has some relation to the facts. One who has closely observed popular movements in America testifies to "the growing belief that mechanical science and invention applied to industry are too closely held by private interests."[3] "An enormous private ownership of industrial mechanism, especially if coupled with lands and mines," forms the gravamen of the complaints. To advert for a moment to the accessory grievance with the view of understanding the main one, can we suppose that in a case such as Ireland was supposed to constitute before the Gladstonian land legislation, the land leaguers would have been content if they had obtained a perfect market in land, an equation of supply and demand undisturbed by hustling or delay, intimidation or cornering?[4] This perfection of the market might have served only to bring out the disadvantage at which the many were placed by the vesting of the complete ownership of land in the hands of a few. The prevailing sentiment about the "enormous private ownership of industrial mechanism" may well be similar. It is true that the expediencies governing "judicial rents" are very different from those which are opposed to the legal regulation of wages. But we are now considering how the matter appears to the many, what regime they can be got to accept. It seems not to be competition pure and simple.[5]

[1] The attribution of a portion of the product to a unit of productive factor is only significant when the unit can be treated as a final increment. *Cp.* Marshall, *Principles of Economics*, note to p. 465, 4th edition. When this condition is not fulfilled,—*e. g.* Professor Clark's *Distribution of Wealth*, p. 326, where "the amount that is attributable to one-half of the capital" ("the capital that is used in the industry") is specified,—this doctrine of attribution becomes perilously like the Austrian doctrine of "imputation," as to which see III, 49.

[2] As argued above, p. 29. See Index s. v. *Entrepreneur*.

[3] Graham Brooks, *The Social Unrest*, p. 122.

[4] Such a market as is analysed in *Mathematical Psychics*, p. 141.

[5] It is possible that competition purified in the manner suggested below might be accepted by moderate trade unionists of the type of Applegarth and Dunning, as to whom see *History of Trade Unionism*, S. and B. Webb.

Are we, then, to abandon the guidance of competition, and follow a higher, an ethical, standard? Does the theory of distribution require a definition of distributive justice? What is justice? The result of Plato's prolonged inquiry would not be satisfactory to the modern asserter of the rights of labour. If a new Socrates were to go about inquiring, what is the ideally just distribution between the employing and employed classes, he would probably find the wisest to be those who confessed their ignorance. As Jevons says, nothing at first sight can seem more reasonable and just than the "favourite saying that a man should have a fair day's wages for a fair day's work. . . . But, when you examine its meaning, you soon find that there is no real meaning at all. There is no way of deciding what is a fair day's wages." [1] It has been well observed that an intuition as to the just rate of wages, the labourer's share of the total product, involves an intuition as to the capitalist's share,—a share which depends on the rate of interest.[2] Can any one seriously pretend that the dictates of a moral sense are clear and decisive in such a matter?

Let it be remembered also that the path of justice is not only dark, but dangerous. Striving to secure the rights of labour, you are very likely to hurt the interests of labour. The action of trade unions by lowering interest and harassing employers may result, as pointed out by Professor Marshall,[3] in checking the accumulation of capital and the supply of business power. The increase in personal capital may indeed compensate for this check, but also it may not. Greater efficiency does not follow higher wages as the night the day.[4]

In view of these considerations it is doubtful whether in the near future an influential majority will aim at setting aside competition. Moreover, even if this consummation were aimed at, it is not likely to be attained. So invincible in human nature is the "propensity to truck," [5] so true is it that, "when one person is willing to sell a thing at a price which another is willing to pay for it, the two manage to come together in spite of prohibitions of King or Parliament, or of the officials of a Trust or Trade Union." [6] Competition is like the air we breathe, which it is not only dangerous, but difficult to exclude.

[1] *Scientific Primer*, chapter on "Wages."

[2] Margaret Benson, *Capital, Labour, and Trade*, chap. xvi.

[3] *Elements of Economics of Industry* (1892), Book VI. chap. xiii.

[4] See the careful statement of the relations by Mr. Pierson in his *Principles of Economics*.

[5] Adam Smith, *Wealth of Nations*, Book I. chap. ii.

[6] Marshall, *Quarterly Journal of Economics*, Vol. XI. (1897) p. 129.

Between two guides, of which neither can be followed implicitly, let us walk warily. On the one hand, let us not aim at impossible ideals. But, on the other hand, let us not deserve the criticism which the advocates of trade unionism have with too much truth directed against "the verdict of the economists" respecting trade unions.[1] Let us not be as trenchant in act as we have been in thought. Let us be cautious in applying our abstract theory to flesh and blood.

To one seeking a representation at once clear and appropriate, the actual conditions of industry present the appearance of a viscous and deliquescent body,[2] not so easy to be treated by simple formulæ as a perfect liquid or a perfect solid. An adequate theory of Distribution must in these days take some account of the action proper to combinations, effecting collective treaties between employers and employed: competition pure and simple no longer constitutes an adequate hypothesis. Eactly how these two principles are to be conceived as coexistent it is premature to state dogmatically: the economist whose aim is to "teach, not preach," to show what is or will be rather than what ought to be, may well hesitate to pronounce on this question. He can at best invent hypotheses which may facilitate the conception of a compromise between the opposed principles of competition and combination. For example, the required compromise might be attained if it were arranged that the agreement between employers and employed under some heads might be settled by collective treaty between combinations, but under other heads by competitive bargaining between individuals,—as the German students in their duels expose only certain parts, not all parts, of the body to the brunt of the combat.[3] To determine what matters should be the subject of treaty would indeed itself require some sort of treaty.[4] But it would be a kind of treaty for which there is good precedent in laws and institutions. For instance, there might grow up, or be enacted by law, the practice that the hours of labour in a trade should be a matter for collective treaty between a trade union and a combination of employers, the particular number of hours to be settled by such treaty, while other terms,

[1] Sidney and Beatrice Webb, *Industrial Democracy*, Part III. chap. i.

[2] *Cp.* J. B. Clark, *Philosophy of Wealth:* "The present state of industrial society is transitional and chaotic. . . . The consolidation of labour is incomplete," that of capital also (p. 148 and context).

[3] *Cp.* J. B. Clark, *op. cit.*, p. 208: "A spirit of Justice is ever standing over the contestants, and bidding them compete only thus and thus."

[4] "No individual competitor can lay down the rules of combat." Sidney Webb, *Contemporary Review* (1889), p. 869.

such as the rate of wages, should be settled by the play of competition.

So far as competition has free play, the received theory of supply and demand, even in its severest mathematical form, would be applicable. Indeed, the severer forms would be peculiarly appropriate in that they do not lend themselves to the contemplation of cornering and other dodges of the market, but assume the "true price" [1] to be worked out honestly. Presumably, the competition which all parties agreed to retain would have to be conducted in a similar spirit. The conditions of the duel, already prescribed, would be further limited by forbidding certain strokes.

A similar regulation may be suggested for the working of an imaginary sort of competition which seems to be contemplated by some who are conversant with the practical problems of industry. Their view appears to be [2] that two combinations might, without resorting to actual competition, agree to accept those terms which would probably result from the play of free competition. In playing this sort of *Kriegspiel*, it might be laid down as a rule of civilised industrial warfare that the workman should not be treated as living from hand to mouth. Suppose him freed from the imminence of starvation for a time at least, and then consider what sort of arrangement of the terms to be settled would constitute a steady flow of the type above described, in which each individual's final sacrifice is normally equivalent to the final utility which he procures thereby.[3] Other rules might be suggested for the working of such imaginary competition.[4]

[1] Condillac's phrase, appropriate to the ideal market above described.

[2] It is difficult to attach any other interpretation to Walker's dicta referred to above. He is presumably supposing that *all* the terms of contract are settled by ideal competition, a limiting case of the regime here suggested that *some* of the terms should be settled by competition, actual or imaginary.

[3] The "method of mutual insurance" practised by trade unions, according to Mr. and Mrs. Webb (*Industrial Democracy*), seems to confer this sort of advantage on its members.

[4] *E. g.* in order to estimate that result, it might be thought consonant to the amount of industrial solidarity actually existing not to treat each individual workman as an economic atom, but rather to suppose comparatively few independent bodies, each formed by the solidification of many individual atoms. Compare T. J. Dunning, *Trade Unions and Strikes* (a work mentioned by J. S. Mill with approval), p. 21, where reply is made to the question, "Why cannot a man sell his labour for what he likes, as a shopkeeper tickets his goods under the price of those of his neighbour?" "The shopkeepers," replies Dunning, "are not obliged to be always together." "But the matter assumes a very different aspect" in the case of wage-earners who work together. Though, as will presently appear, a preliminary use of the sort of potential competition which has just been described may be required.

But it may be questioned whether the method admits of precision, for a reason urged by Mr. L. L. Price with reference to a proposed principle of arbitration, "that the arbitrator should endeavour to award such wages as would be attained if combination on either side were absent." "Where is the arbitrator to discover this ideal standard?" pertinently asks Mr. Price.[1]

The terms forming the subject of a collective treaty would be settled by a method essentially different from competition. For instance, in the case above proposed, the length of a working day, let there be a law removing this article from the category of terms which are to be settled by the play of competition between individuals. Those who hold that such a law is based on the utilitarian first principle, the greatest happiness of those concerned, —here the citizens who have enacted the law,—will be prepared for the further suggestion that the particular number of hours to be settled will also be regulated by the utilitarian first principle, only that those concerned, whose maximum advantage constitutes the criterion, are not now the citizens,—if the citizens generally have no interest in the particular number of hours in the trade,—but only the parties to the distribution, the members of the contracting combination. That this undergrowth of utilitarianism may, like the parent tree, prove fruitful, has been argued elsewhere.[2] Here it need only be repeated that, when the utilitarian arrangement is defined as the basis of conciliation between self-interested parties to a contract, it is presupposed that both parties gain by the contract:[3] that it does not seem to either party to be their interest, rather than accept such an arrangement, to give up dealing at all with the other party—seek, it may be, some third party, some other employment of their capital and labour,[4] or at least to defer agreement with the other

[1] *Economic Science and Practice*, p. 198 and context.

[2] II. 101, and *Mathematical Psychics*, p. 53.

[3] Consider the weighty passage referring to the principles on which courts of arbitration and boards of conciliation should act, in Marshall's *Economics of Industry* (1879), Book III. chap. viii. § 2: "They must not set up by artificial means arrangements widely different from those which would have been naturally brought about," *et seq.* Compare Marshall's Preface to (L. L. Price's) *Industrial Peace*, p. xxiii: "The arbitrator is compelled to take some account of the fighting forces of the two sides; the necessity to be practical may compel him to go further than he would otherwise have done away from an absolute standard of fairness."

[4] In the technical terms of *Mathematical Psychics* the *utilitarian point* in the *contract-curve* must not be outside the points at which that curve is cut by the *indifference curves.* It is significant that this abstract representation is adapted to the first rather than the second of the two cases, in which the utilitarian arrangement would not be accepted,—the case, for example, in which the capitalist combination refuses the arrangement, because, considering it as

party, in view of the probability that they will reduce their terms.[1]

The rationale of conciliation thus presented will doubtless not commend itself to many who accept substantially identical principles invested in a different form. Uniformity is not to be expected in the enunciation of first principles. The vital tenet is that each party must take account of and enter into the wants and motives of the other party. When competition is no longer umpire, the economist must abandon—if he ever maintained—the position of extreme *solipsism* which Jevons in a solitary but remarkable passage has propounded :—

Every mind is thus inscrutable to every other mind, and so no common denomination of feeling seems to be possible. . . . The motive in one mind is weighed only against other motives in the same mind, never against the motives in other minds. Each person is to other persons a portion of the outward world. . . . Hence the weighing of motives must always be confined to the bosom of the individual.[2]

Jevons himself has not remained consistently on this pinnacle of solitude. It is abandoned by economists in general in the received theory of taxation, founded, as Mill says, on "human wants and feelings."[3] Self-regarding self-interest, the gospel of Adam Smith, is not alone sufficient for industrial salvation : a leaf must be taken from his older and less familiar testament, of which the cardinal doctrine was *sympathy*. Sympathy does not necessarily imply sentimental attachment : sympathy, according to Adam Smith, is the basis of a not very sociable emotion,—ambition. A distinguished psychologist has not hesitated to pronounce "sympathy compatible with dislike."[4] It is, then, no counsel of perfection to cultivate sympathy, in the sense of mutual understanding, between the parties to distribution. No Utopian eradication of self-love is contemplated. It may be

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permanently at work, they would be worse off than if they were to transfer their capital to some other field of enterprise; not the case in which they defer making an agreement for strategic reasons, because, being better supplied for a siege, so to speak, than the other party, they hope to reduce them in case of a strike to submission. Compare what was said above as to the advisability of not admitting this kind of strategy into industrial combat waged under ideal conditions.

[1] Compare Marshall, *Economics of Industry*, *loc. cit.* : "Mischief almost always results in the long run from an award which gives to one side terms much worse than those which it knows it could obtain by a strike or a lock-out."

[2] *Theory of Political Economy*, edition 3, p. 14.

[3] *Political Economy*, Book V. chap. ii. § 4.

[4] Bain, *Emotion and Will* (Table of Contents).

hoped, indeed, that through the practice of conciliation, in the course of generations, the dispositions of which the gratification constitutes self-interest may become more social, so that, for instance, an advantage founded on the extreme privation of others would not appear desirable to the capitalist employer of the future. But such "moralisation" of the saving classes, though it may be expected, need not be postulated for the working of conciliation. Intellectual sympathy alone might effect much. The arts [1] by which the sympathetic imagination may be cultivated form a supremely important topic, but one which hardly falls under the *theory of Distribution.*

*Note referring to p. 24.*

[On the remuneration for risk some additional light is derivable from Mr. Keynes' great treatise on Probability; where he shows that mathematical expectation—the product of advantage and the probability of obtaining it—is not the measure of expediency (ch. xxvi. p. 311 *et seq.*; discussed by the present writer in *Mind*, 1922, vol. xxxi. p. 276 *et seq.*). The motives of the entrepreneur may be illustrated by the position of Paul in the classical problem which Mr. Keynes thus restates: "Peter engages to pay Paul one shilling if a head appears at the first toss of a coin, two shillings if it does not appear until the second, and in general $2^{r-1}$ shillings if no head appears until the $r$th toss. What is the value of Paul's expectation?" If the number of tosses is limited to a finite number $n$, the mathematical expectation is $\frac{1}{2}n$. But, if $n$ is large, no sensible person would give anything like that sum for the chance. Now Paul may be taken as typical of the entrepreneur. Peter in this case may fix what Paul must pay for a trial—corresponding, say, to the outlay on factors of production required for a unit of product. But Paul will have a say as to the amount which he stands to win by that outlay. Say the payment is $\frac{1}{2}n$ shillings or pounds, $n$ not now indefinitely large; Paul

[1] For example, co-operation, as many economists have pointed out, would have among its good effects that of enabling workmen to realise the position of employers. Again, the training of future business men in economics at the universities, as Professor Marshall has lately urged, would tend to develop the sympathetic use of the imagination. "It has been found," he says, "by experience in England and in America that the young man who has studied both sides of labour questions in the frank and impartial atmosphere of a great university is often able to throw himself into the point of view of the working-men and to act as interpreter between them and persons of his own class with larger experience than his own." See his address on "Economic Teaching at the Universities," published in the review of the Charity Organisation Society, January, 1903, noticed in the Economic Journal, Vol. XIII. p. 155, and his *Plea* for the creation of a curriculum in economics (addressed to the Cambridge Senate), noticed in the Economic Journal, Vol. XII. p. 289.

Compare the expressions in the *Report of the Anthracite Coal Commission*, U.S.A. (1903), on the importance of "a more conciliatory disposition in the operators and their employees."

will demand a higher prize than the bare actuarial $2^{r-1}$; unless he is a fatuous gambler (*cp.* Marshall, *Principles*, Bk. V. ch. vii. § 4, and p. 613, note, 5th edition; and Pigou on uncertainty-bearing). At what terms above the actuarial limit Paul will touch the point of indifference, what is his demand-schedule in respect of such transactions, depends upon his mentality, his "dispositions," in the phrase of Walras relative to supply and demand in general. Thus the share of the entrepreneur in the product equally with the share of the workman depends on the play of demand and supply. It is no more predetermined than the wage-fund.]

## (C)

# THE LAWS OF INCREASING AND DIMINISHING RETURNS

[THE following article, published in the ECONOMIC JOURNAL 1911, formed (the greater part of) an introduction to a series of articles entitled *Contributions to the Theory of Railway Rates;* which was discontinued after 1913. This discussion of Increasing and Diminishing Returns which initiated the series is almost as applicable to a regime of Competition as to one of Monopoly. But its original use as an introduction to an essay on a certain kind of monopoly is occasionally discernible.

There are here distinguished two definitions of increasing (and of diminishing) returns, which are respectively appropriate according as the magnitude of which increase (or diminution), is predicated is marginal or total. It is proposed to remove an ambiguity, the existence of which in so familiar a subject might have been deemed impossible, but that a similar confusion occurs in the matter of " sacrifice " incurred through taxation as pointed out below II. 115, and perhaps even (as there suggested) in a still higher sphere. The definition based on marginal production is here distinguished as *primary*, preferred as more directly related to theory of *maxima*. The use of the *secondary* definition is shown to present difficulties in the important case of plural factors of production.

The *conceptions* having been made clear, some of the *propositions* of which they form the terms are restated. The advantages of Production-on-a-large-scale and of Division-of-labour are classified.

There follows an inquiry into the meaning and properties of Joint Cost, and other cognate conceptions. In the course of the inquiry there comes into view that case of which the title to Joint Production has been disputed between Professor Pigou and other high authorities (see Index s.v. *Joint Production*).]

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Increasing Returns and Joint Cost admittedly play a great part in economics; but what that part is has been questioned.

1. *Law meaning concept.*—A first difficulty is occasioned by a certain ambiguity in the use of the word "law" in such phrases as the "law of increasing [or diminishing] returns." The word "law" in this connection is used sometimes in the narrow sense of a quantitative relation; sometimes in the larger sense connecting that conception with some other attribute. Thus the "law of diminishing returns" may stand either for the conception of decrease in the rate at which production increases, or for a proposition connecting that conception with the increase in the number of producers under certain circumstances. Accordingly Mr. Flux well distinguishes the "definition or statement of the law" from the "assertion of its applicability." [1] So Mr. Maurice Clark, in his philosophical study on *local freight discriminations*, appears to treat the "law of joint cost" as equivalent to the "term 'joint cost.'" [2]

This sort of ambiguity is not unknown in other sciences. Thus the "law of error" is sometimes used to denote the relation between frequency and deviation which is expressed by a certain well-known curve (or *function*), and sometimes for the proposition that the said quantitative relation tends to be realised approximately in certain circumstances of general occurrence. Even in physical science such a phrase as "the law of the inverse square" is not improperly, I think, sometimes used to denote simply a certain relation between the magnitude of a force and the distance at which it operates, a conception which may be predicated of different forces—now the attraction of gravitation, and now the repulsion of electricity.

We may begin by interpreting the laws in question in the narrower sense. Let it not be objected that this is a matter of verbal definition. For, as Sidgwick reminds us, some of the most important inquiries have taken the form of a search for definitions.[3] More reassuring to some may be the reflection that even in modern physical science half the battle often consists in obtaining what Whewell well described as clear and appropriate conceptions, "ideas distinct and appropriate to the facts." [4]

2. *Provisional definition.*—The definition of the law as a *term* may be gathered from an authoritative statement of the law as a

[1] Palgrave's *Dictionary*, Article on Law, p. 583.

[2] Pp. 28, 29, 30.

[3] *Political Economy*, Book I. ch. ii. § 1, suggesting the application of this Platonic method to economic investigations; *cf.* Book II. ch. iv. *note* (ed. 3).

[4] *History of the Inductive Sciences*, Book I. ch. iii. § 2 *et passim*. Mill, while refusing to "Colligation" the title of Induction, does not deny its supreme importance.—*Logic*, Book II. § 4.

*proposition.* The essential attribute is presented in the following passage—which want of space compels me to separate from the explanation and limitations in the context—from Dr. Marshall's statement of the law of diminishing return with respect to agriculture :—" Our law states that sooner or later . . . a point will be reached after which all further doses will obtain a less proportionate return than the preceding doses." [1] So with respect to manufactures, Dr. Marshall says :—" If a manufacturer has, say, three planing machines, . . . after they are once well employed, every successive application of effort to them brings him a diminishing return." [2] So Mr. Flux discriminates between Increasing or Diminishing Return by the change in "marginal expenses per unit." [3] We are countenanced, I think, by good authority in adopting the following provisional definition of the terms. When on the application of two successive equal doses of productive power, the increment of product due to the first dose is less than the additional increment due to the second, the law of increasing returns is said to act; and conversely it is a case of diminishing returns when the increment due to the first dose is greater than the increment due to the second.

The attribute which I regard as essential may be illustrated by a numerical example. In the accompanying table the first two columns are borrowed from an example given by Professor Carver.[4]

TABLE I.

| Days' labour of man with team and tools. | Total crop in bushels. | Increments due to successsive doses. |
|---|---|---|
| 2 | 0 | 0 |
| 5 | 50 | 50 |
| 10 | 150 | 100 |
| 15 | 270 | 120 |
| 20 | 380 | 110 |
| 25 | 450 | 70 |
| 30 | 510 | 60 |
| 35 | 560 | 50 |
| 40 | 600 | 40 |
| 45 | 630 | 30 |
| 50 | 650 | 20 |

[1] *Principles of Economics,* sixth edition, p. 153.
[2] *Op. cit.* p. 168.
[3] *Economic Principles,* p. 47.
[4] See Prof. Carver's *Distribution of Wealth,* ch. ii. p. 58, and compare his article in the ECONOMIC JOURNAL, Vol. XVIII. A part of Prof. Carver's third column, not shown in my Table I., is reproduced (with some additional matter) in my Table II. I have also taken the liberty of substituting in his first column for his figure 1 the figure 2.

I have added the third column to illustrate the distinction between Increasing and Diminishing Returns according to the definition here provisionally adopted. The figure in the third column distinguished by heavy type, viz. 120, marks the point up to which returns are increasing and after which they become diminishing.

If we plot a set of figures like those above given and exhibit the relation between the figures in the first and those in the second column in the form of a curve, it will be found that the character of the law (whether " increasing " or " diminishing ") depends on the character of the curve in respect of concavity or convexity.*

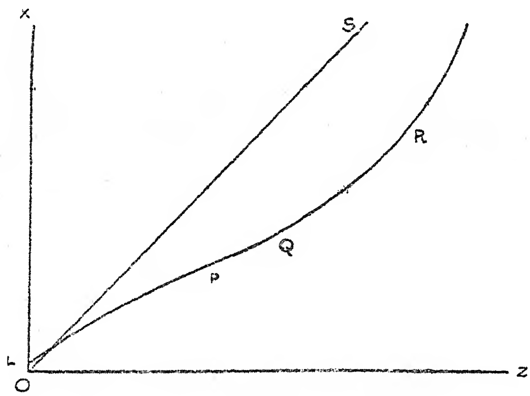


FIG. 1 A.

In Fig. 1 A the vertical axis $OX$ is taken to represent outlay; each quarter of an inch on this ordinate denoting a " dose " of five days' labour. The corresponding returns are represented by the axis $OZ$; each quarter of an inch on this abscissa denoting fifty bushels. It will be observed that the curve is concave (with respect to the horizontal) up to the third dose, the point $P$, while Increasing Returns acts; and becomes convex when Diminishing Returns sets in. In Fig. 1 B representing the same data, with the directions of the co-ordinates interchanged, Increasing and Diminishing Returns correspond respectively to the con-

* If $z$ denotes the (amount of) product and $x$ the (amount of) factor used in the production; the curve in Fig. 1 A will be convex, and the curve in Fig. 1 B concave (towards the abscissa), when $\frac{d^2x}{dz^2} > 0$, or $\frac{d^2z}{dx^2} < 0$ (which conditions come to the same, since $\frac{dz}{dx}$ is supposed positive). For

$$\frac{d^2z}{dx^2} = \frac{d}{dx}\frac{dz}{dx} = \frac{d}{dx}1\Big/\frac{dx}{d} = \left(\frac{d^2x}{dz^2}\cdot\frac{dz}{dx}\right)\Big/\left(\frac{dx}{dz}\right)^2.$$

vexity and concavity of the curve with respect to the horizontal axis.[1]

3. *Doses of various size.*—In order to make our definition precise it is often necessary to specify the *magnitude* of the doses successively applied. Otherwise it may happen that *both* Increasing and Diminishing Returns may truly be predicted of the same circumstances.[2] This is a paradox familar to those who are conversant with the application of the differential calculus. It depends on the circumstance that the orders of magnitude which may be neglected are different according to the different purposes

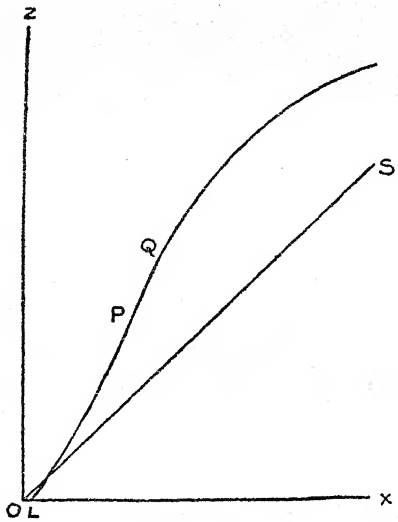


FIG. 1 B.

contemplated. It is thus—to use Clerk-Maxwell's illustration [3]—that the heterogeneities in the structure of a mound of gravel, which are negligible from the point of view of the railway contractor, may be all-important to the worm. For a like reason the surface of a mountain at any assigned point, say that which is exactly underneath the centre of gravity of an ascending mountaineer, may appear to him, while he surmounts the rough surface with long strides, to be shaped like a dumpling, concave with respect to the plane of the horizon; but to the beetle creeping up a cup-shaped cavity, convex.

[1] Of the two nomenclatures, here as usual treated as equivalent, namely increasing [or diminishing] return and diminishing [or increasing] cost, the former seems more appropriate to construction B, the latter to construction A.

[2] *Cf.* Marshall, *Principles*, ed. v. p. 159 and context.

[3] In a passage quoted II. 389.

Similarly in the example above given, if we use doses each consisting of *twenty-five* days' labour, the character of increasing return will no longer be presented. For the return due to the first dose will now be 450; the return due to the second dose 200. Even if we use doses each consisting of *fifteen* days' labour we do not find increasing returns; the returns to the successive doses being 270, 240, 120. So when the passenger traffic on a railway is increased, for small additions requiring only additional carriages on the trains already running, the case may be one of diminishing cost, increasing return; but for large additions requiring additional trains on an already crowded track, the case may be one of increasing cost, diminishing return. Yet again, when the increase of traffic is such as to call for a new track, a change of this magnitude might well present the law of increasing return. In such cases, then, in order that the predication of either law of return should be significant, it is necessary that the size of the dose should be explicitly assigned, if not implied by the context.

4. *General definition.*—So far we have tacitly supposed the two successive doses to be equal in magnitude. But now, removing this restriction, we may define that when on the application of two (not in general equal) doses of productive power the increment of product due to the two doses has to the increment of product due to the first dose alone a ratio greater than the ratio which the sum of the two doses has to the first dose, Increasing Return acts; [1] and conversely if the former ratio is *less* than the latter, Diminishing Return.[2] For example, in the numerical instance above given we may say that after the stage at which thirty-five days' labour (supplemented by team and tools) have been applied, the law of diminishing return acts, since one dose (of five days, etc.)—added to six such doses—produces 60 bushels, while three added doses produce 60 + 50 + 40 = 150, and the ratio of 150 to 60 is less than the ratio of 3 (doses) to 1. This definition appears to agree with the expressions employed in

[1] In other words (used by the present writer, ECONOMIC JOURNAL, Vol. XIX. p. 293) "the law of increasing cost or diminishing returns holds good when the ratio of the last increment of cost to the last increment of produce is greater than the ratio of the penultimate increment of cost to the penultimate increment of produce; with a corresponding statement for the law of diminishing cost (increasing returns)."

[2] Let $z = f(x)$. At any point $x_1$ increasing or diminishing return acts according as

$$\frac{f(x_2) - f(x_0)}{f(x_1) - f(x_0)} > \text{ or } < \frac{x_2 - x_0}{x_1 - x_0};$$

where $x_0 < x_1 < x_2$.

many standard treatises. Thus Professor Nicholson predicates of "increasing returns" that "under certain conditions every additional unit of productive power gives more than proportional returns";[1] with a corresponding definition of diminishing returns. So Professor Seager states with respect to diminishing returns in agriculture, that, after a certain point, "applications of labour and capital yield less than proportionate returns in product."[2] Such expressions often leave it doubtful whether they were intended to refer to the general definition which has been here enunciated, or only to the particular, though extensive, species which is now to be distinguished.

5. *Division into two species.*—Our general definition comprehends a particular and limiting case in which the difference of degree almost amounts to a difference of kind. This case is constituted by taking as the first of the two successive doses the whole of the labour-and-capital or productive power measured from zero; the second dose being larger or smaller according to the purpose in hand.[3] Thus understood, the definition comes to this, that the law of increasing return acts when the *average* product per unit of productive power applied increases, with the increase of productive power (by an amount that is of an assigned order of magnitude); and the law of diminishing return, in the converse case. The definition thus presented may be distinguished as *secondary;* the general definition, exclusive or irrespective of this limiting case, being called *primary.*[4] It should be observed, however, that many high authorities seem to give precedence to that definition which is here described as secondary. Thus Walker makes the distinction between increasing and diminishing returns, with reference to a given tract of land cultivated by ten labourers, to turn upon the question whether or not, if two new labourers are taken on the twelve raise more per man than the ten could do. Similarly Professor Seligman[5] and other eminent American economists.[6] So Professor Carver, with reference to the instance above cited from him, understands that "increasing returns stop and diminishing returns begin at the point where twenty days' labour are expended in the cultivation of the field"[7]—that is at the fourth dose (of five days' labour), not as according to our definition the

[1] *Principles of Political Economy,* Vol. I. p. 172.

[2] *Introduction to Economics,* 1904, p. 114.

[3] Putting $x_0 = 0$ in the formula given in note 2, p. 66.

[4] *Cf.* below, p. 152.

[5] *Shifting and Incidence of Taxation,* quoted by the present writer, *loc. cit.*

[6] *E.g.* Bullock, *Elements of Economics,* p. 76.

[7] *Distribution of Wealth,* p. 58.

third dose—because up to that point the average product per dose (or what comes to the same, "per day's labour") goes on increasing.

To exhibit the distinction more clearly, I suppose Table I. to be graduated more finely in the neighbourhood of the transition

TABLE II

| Days' labour of man with team and tools. | Total crop in bushels. | Increments due to successive doses. | Bushels per day's labour. |
|---|---|---|---|
| ... | ... | ... | ... |
| ... | ... | ... | ... |
| 13 | 220 | ... | 16·92 |
| 14 | 244 | 24 | 17·43 |
| 15 | 270 | **26** | 18 |
| 16 | 294 | 24 | 18·38 |
| 17 | 317 | 23 | 18·65 |
| 18 | 339 | 22 | 18·83̇ |
| 19 | 360 | 21 | 18·95 |
| 20 | 380 | 20 | **19** |
| 21 | 396 | 16 | 18·86 |
| ... | ... | ... | ... |
| ... | ... | ... | ... |

from Increasing to Diminishing Returns; and I add a fourth column [1] corresponding to the second definition. It will be seen that for a considerable tract of values—corresponding to the portion of the curve in Fig. 1 A between the points $P$ and $Q$—the primary and secondary definitions do not come to the same. The difference in connotation involves a sensible difference in denotation.

The grounds on which precedence is claimed for the primary definition will presently appear.

6. *Significance of price.*—So far we have mostly measured the producing doses and the resulting product in *kind*.[2] But no essential difference in classification is introduced when we take money as the measure; provided that the prices, both of the product and the factor of production, remain constant while the amounts are varied. For the change thus introduced is simply to multiply the axes representing outlay and return, the $Ox$ and $Oz$ of Fig. 1 each by a constant: to change the scale of both

[1] Corresponding to Prof. Carver's *third* column.

[2] *Cf.* Marshall, *Principles*, ed. v. p. 152: "the return to capital and labour of which the law [of Diminishing Return] speaks is measured by the *amount* of the produce raised independently of any change that may meanwhile take place in the *price* of produce."

the abscissa and the ordinate.[1] But such a change does not alter the character of a curve in respect of convexity or concavity. If it was convex or concave at any point before the change, the transformed curve will have the same character at the corresponding point. The character of the return, whether increasing or decreasing, in the primary sense, depends on the material conditions of production, not on the accidents of price. With respect to the distinction in the secondary sense, we may employ a theorem given in my former treatment of the subject,[2] that in such a figure as our 1 A above, if a tangent (not shown in the figure) is drawn from the origin to the curve, the point of contact is the limit at which the returns cease to be increasing in the secondary sense and become decreasing. This relation, too, may be considered as an *invariant*, not varying with a change of scale.

But money can no longer be ignored when we consider price as varying with the amount put on the market by the individual entrepreneur; as it is proper to conceive in a regime of monopoly. We must now distinguish $z$ the amount of product in kind due to the employment of the factor $x$,[3] and $\zeta$ the money-value of that product depending on the law of demand.

7. *Factors and other coefficients.*—In general, we may presume that, as shown above in Fig. 1, to any assigned amount of outlay there corresponds a definite amount of product, and conversely. In this presumption it is taken for granted that the entrepreneur applies his outlay to the best of his ability [4] in order to obtain the greatest possible profit. To that end he may have to adjust any number of variables, such as the time of trains, the place of porters, and so forth. We ought to distinguish this sort of coefficient, which does not enter into the expression for outlay * from factors-of-production usually regarded as, in the phrase of a judicious writer, "factors of expense." [5]

[1] If in Fig. 1 A the ordinate represent not $x$ the amount of a factor, but $\xi$ the money value thereof, the curve will then be a cost-curve of the kind employed by Auspitz and Lieben.

Similarly in Fig. 1 B the abscissa may be taken to represent outlay in money.

[2] *Loc. cit.*, p. 294.

[3] Supposed to be purchasable by the monopolist at a constant price.

[4] *Cf.* Marshall, *Principles*, ed. v. p. 152: ". . . taking farmers as they are with the skill and energy which they actually have." *Cf.* also the passage cited in the sequel (p. 97), with reference to Prof. Carver's views.

* These *gratuitous* coefficients may be identified with the "parameters" $u, v, w$ . . . which Dr. Zotoff in his elaborate note on the Mathematical Theory of Production (ECONOMIC JOURNAL, Vol. XXXIII. p. 115) introduced and eliminated.

[5] Johnson (and Huebner), *Railway Traffic and Rates.*

Here might appropriately follow the discussion of plural factors of production. But it is better first, still with special reference to a single simple factor, to advert to the grounds on which different definitions are preferred.

8. *The two species compared.*—Things which are insignificant for the purposes of action and pleasure do not obtain names. What then is the purpose with reference to which the names now in question have been imposed? The essential fact, I submit, is that the attribute designated Diminishing Return is the criterion of a *maximum;* not only of a quantity such as $z$, the product considered as a function of $x$, the factor used, but also of a quantity such as $bz - cx$ (where $b$ and $c$ are constants), denoting the net product.[1]

Moreover, it is to be conceived that an analogous condition is fulfilled by the *gratuitous* coefficients above noticed,[2] though the vocabulary of the economist may fix attention on the paid factors of production. For instance, in the case of a given amount of labour and capital to be applied to an optional amount of land,[3] the condition which must be fulfilled by the law of production in order that the product should be a maximum is the same whether the land is free, or subject to a rent per acre.[4]

How comes it, then, that the *secondary* definition is so largely employed by economists? For one reason, there is often no difference in the denotations corresponding to the different connotations. This occurs when the cost-curve represented in Fig. 1 A is convex (to the abscissa), *ab initio.*[5] This coincidence of fact may explain the frequent use of different definitions by the same writer.[6]

[1] *Cf.* below, p. 74. In the abstract $b$ and $c$ may be not prices, but quantities of some commodity other than money, in particular the commodity produced.

[2] Above, subsection 7.

[3] As in Prof. Carver's instructive example above cited.

[4] Let the product be $z, = f(x, l)$, where $x$ is the amount of working capital, $l$ of land employed; and let the net product be $f(x, l) - c_1x - c_2l$, where $c_1$, $c_2$ are constants (*cf.* note 1 above). The criterion of a maximum, namely, that the second term of variation should be thoroughly negative, is the same whether $c_2$ is zero or not.

[5] *Cf.* below, p. 157.

[6] The coincidence is thus affirmed by one who was among the first to discern the principle of Diminishing Returns—West :—"each additional quantity of work bestowed on agriculture yields an actually diminished return, and, of course, if each additional quantity of work yields an actually diminished return, the whole of the work bestowed on agriculture in the progress of improvement yields an actually diminished proportional return."—*Essay on the Application of Capital to Land*, pp. 6–8, quoted by Prof. Cannan, ECONOMIC JOURNAL, Vol. II. p. 63.

But this identity is not always to be supposed. Rather, the curve in Fig. 1 A is typical of many modern industries in which an initial outlay is required. What is the rôle of the *secondary* definition in such cases? Let us consider this nice question with reference to three kinds of economic regime: (*a*) perfect competition, (*b*) monopoly practised by a perfectly self-interested monopolist, (*c*) monopoly practised, or at least regulated, by the State.

(*a*) In the first case it is proper to suppose a constant price at which each entrepreneur strives to sell that amount of product which brings him in a maximum profit. If in Fig. 1 A the constant price is represented by the inclination (to the abscissa) of a straight line through the origin [1] (the axis $OZ$ now representing the money-value of any quantity of product $z$), then the amount produced by an individual whose cost-curve [2] is $OLPQR$ will be the abscissa to the point on the curve, which is such that a tangent to the curve at that point is parallel to the line $OS$; provided that the curve is convex (to the abscissa) at that point.[3] Now at first sight it might appear that this condition could be satisfied by the convex portion of the curve in Fig. 1 A, between $P$ and $Q$, if the price were suitable. But the condition will be found to imply that the total gain obtained from the production is less than the total loss incurred; which is, normally and in the long run, absurd. Accordingly, we are concerned (in a regime of competition) only with that part of the curve which fulfils the secondary as well as the primary definition, the tract beyond $Q$.[4] When we speak of Increasing Return in the present connection we are mostly not thinking of the concave portion of a curve

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Mr. Flux, whose book has been cited above on behalf of the primary definition, seems to adopt the secondary one in his article on "Law" in Palgrave's *Dictionary*.

[1] In accordance with the Auspitz-and-Lieben construction, noticed in the ECONOMIC JOURNAL, Vol. XVII. p. 226.

[2] In the sense above explained. Good examples of (the materials for) such a curve are given by Cunynghame (*Geometrical Economics*); referred to by the present writer (ECONOMIC JOURNAL, Vol. XV. p. 67), and distinguished from a supply-curve, individual or other.

[3] $R$ in Fig. 1 A, is meant to represent this point, corresponding to the *seventh* dose of labour (thirty-five days of labour), in accordance with the data of Table I. A.

In Fig. 1 B (the abscissa of any point on) $OS$ may stand for the cost, the amount of outlay $x$ multiplied by a constant; while the ordinate to the curve is the total yield (in money or other scale commensurate with the cost).

[4] For a more complete analysis the reader is referred to Prof. Pigou's stupendous article on "Producers' and Consumers' Surplus" in the ECONOMIC JOURNAL, Vol. XX. [Restated in *Wealth and Welfare*; with reference to which see II., 323, 433, and contexts.]

such as that in Fig. 1 A; but of something quite different, which might be illustrated as follows:—Let the curve in Fig. 1 A represent the cost-curve for an individual typical entrepreneur. With the increase of production in virtue of "external economies," the curve, or that tract of it with which we are concerned, may be lowered as a whole in such wise that each amount of product, $x$, corresponds to a lower cost. Similarly, Diminishing Return now has a signification other than the convexity of a curve such as that in Fig. 1 A.

It may be worth remarking that when we contemplate the working of a competitive regime as bearing on the interest of the community, from the point of view of the philosophic statesman, then we welcome the phenomenon of Increasing Return (or deprecate its contrary) as tending to (or from) some quantity which it is proposed to maximise.[1] But the criterion of such a maximum is analogous to our *primary* conception.

(*b*) When we leave the case of perfect competition, the sort of return which is diminishing in the primary but not the secondary sense—the (convex) tract of the curve in Fig. 1 A between the points $P$ and $Q$—becomes more significant. Suppose that the *general* expenses of a Company, like that of the canals in France, were defrayed by the Government, then, even though the ruling price, determined, say, by competition, were inadequate to the total expenses, it might be the interest of the Company to produce an amount between (the amounts corresponding to) $P$ and $Q$. Something similar occurs in the case of two competing railways obliged in the struggle for survival to leave out of[2] account past expenses of construction. As we continue to remove the conditions proper to the regime of competition, the importance of the point $Q$, at which Diminishing Return in the secondary sense sets in, becomes less conspicuous. Suppose that in the case put by Professor Carver[3] the farmer has a limited amount of capital-and-labour, say thirty-four days' labour, to apply to plots of land which he can have for nothing. The arrangement which he will find most profitable is to cultivate two such plots of land, applying to each seventeen days' labour; since thus on the assumptions of our Table II. he would produce (twice 317 =) 634 bushels; whereas, by applying the whole thirty-four days' labour

[1] J. S. Mill sometimes expressed himself as if the greatest *average* well-being was the *summum bonum*. But the better opinion, I think, is that of the philosophic Sidgwick that the end of political action is to maximise the *quantum* of happiness.

[2] The case of *duopoly*; below, p. 117.

[3] *Distribution of Wealth*, ch. ii.

to one plot, he would have produced less than 560 bushels (the produce of thirty-five days' labour according to Table I.).

In ordinary monopoly the outlay would not be limited thus absolutely, but by the necessity of limiting the production in order to keep up the price. The limit may be exhibited by one of Auspitz and Lieben's Constructions. In our Fig. 1 A let the curve represent cost to a monopolist of any amount $z$ produced. And substitute (in imagination, not shown in the figure) for the straight line $OS$ a curve passing through $O$ concave to the abscissa; the ordinate representing the total value of the abscissa, $z$. Then the point of maximum profit to the monopolist may well prove to be a point in the tract between $P$ and $Q$. Thus it by no means seems to be a universal truth that "with a given road-bed and with a given equipment in the way of depôts, offices, machine shops, etc., and with a given labour force, an increase in the rolling-stock will, between rather wide limits, enable the road to carry more freight and passengers; but this increase in its capacity will not be proportional to the increase in the rolling-stock." [1] If we represent the outlay on the "given road-bed" by $OL$ in Fig. 1 A (not drawn to scale), and the outlay in rolling-stock by increments along $OX$ above $L$, it is not certain that this outlay will be pushed up to a point corresponding to $Q$ in the figure, as the above statement implies. If the demand for passenger-service is very inelastic, it might be the interest of the Railway to restrict the supply within such limits that the increase of carriage room would present Increasing Return (in the secondary sense contemplated). Nay, it is quite possible that Increasing Return in the primary sense may rule; the monopolist may arrest production at a point below even $P$ in our figure,[2] a point beyond which he would lose by the falling-off of demand more than he would gain in cheapness of production.[3] This is possible but not probable. For there is a correlation—though not an identity—between the criteria of maximum

[1] Carver, *Distribution of Wealth*, ch. ii. p. 86. *Cf.* p. 88: "An increase of the rolling-stock would (except in very exceptional circumstances) increase, but not proportionately, the carrying capacity of the road."

[2] As pointed out by the present writer, ECONOMIC JOURNAL, Vol. XVII. p. 236.

[3] For instance, it is possible that railways in America are deterred from lowering passenger fares, not so much by the cost of increased accommodation as by the belief that the demand would not keep up (*cf.* Johnson and Huebner, *Railway Traffic and Rates*, Vol. II. p. 207). They may be wrong in that belief as Weyl and others urge (*cf.* Johnson, *American Railway Transportation*, p. 150); but it is possible that they may be right. But owing to the circumstance of Joint Cost (for freight and passenger) and Discrimination (between passenger fares) a clear-cut concrete example is not to be expected.

(1) for the net profit of the monopolist (affected by selling price), and (2) for the amount of product in kind (not so directly affected).[1] While the primary conception is thus less important in a regime of monopoly than in one of competition, the secondary conception is *much* less important. It is even fallacious. The suggestion which has been made by authors of note that the quantity which the monopolist seeks to maximise is the *average* return to his outlay—the rate, not the amount, of profit—is a misleading suggestion.

(*c*) The peculiarity of State Monopoly is that it seeks—to some extent at least [2]—to maximise not pecuniary profit in the ordinary sense, but the advantage of customers measured in money, the collective Consumers' Surplus. Now, the characteristic of such a maximum is a relation of the kind designated by the primary rather than the secondary definition.

Upon the whole, I think, there abide both the primary and the secondary definition; and the greater of these is the primary.

9. *Plural factors.*[3]—These comparisons may be transferred to the case of plural factors of production, *mutatis mutandis.*[4] The

[1] Let the net profit be $\zeta - x\pi$, where $\zeta$ is the money value of the product obtained by the application of the amount (in kind) $x$ of the factor of production, $\pi$ is the (supposed constant) price of the factor. Also $\zeta = zp$, where $z$ is the amount of product in kind, and $p$ is the price thereof (supposed liable to be varied by the monopolist):

$$\frac{d^2\zeta}{dx^2} = \frac{d^2z}{dx^2}p + 2\frac{dz}{dx}\frac{dp}{dx} + z\frac{d^2p}{dx^2}.$$

Accordingly, if $\frac{d^2z}{dx^2}$ is negative (Diminishing Returns in the primary sense ruling) *probably* $\frac{d^2\zeta}{dx^2}$ is negative. For the latter quantity is equal to the former ($\times\ p$) *plus* two quantities, one of which is known to be negative $\left(\frac{dz}{dx}\text{ being positive, }\frac{dp}{dx}\right.$ negative), while the sign of the other is quite unknown. The probability is of the kind which I have described as *a priori* in former numbers of the ECONOMIC JOURNAL (in particular, Vol. XX. p. 287–8). I should like to add to the instances there given Professor Pigou's (virtual) recognition of the principle when in his important communication to the Poor Law Commission (Appendix lxxx.) he argues that "unknown facts are as likely to conform as to conflict with known facts." [For further references see Index, s.v. *A priori Probabilities.*]

[2] In making this qualification I have in mind Dr. Marshall's doctrine of "compromise benefit."

[3] The plural factors are here considered as forming one product. The case of several products with several factors falls to be considered under the head of *Joint Production.*

[4] Among the changes required is the introduction of a new symbol, $\kappa$, to denote the sum of the money-values of the factors. By means of the production-function $z$, $= f(x, y, \ldots)$, where $x, y \ldots$ are amounts of the factors in hand, we may determine the values of $x, y \ldots$ which give the maximum value of $Z$, the net profit, $(= \zeta - \kappa)$ for any assigned value of $\kappa$; and thence obtain $Z$ as a

required changes tend to augment the difference between the two definitions, to enhance the precedence of the primary. Beginning with two factors of production, let us measure their amounts in kind along two axes, $OX$ and $OY$. And let a perpendicular to the plane of those axes (say the plane of the paper) at any point in the plane, designated by the co-ordinates $x$ and $y$, represent by its height $z$ the product in kind resulting from the employment of $x$ and $y$ in the best available ways. Then the terms Increasing and Diminishing Return are to be defined by the character of the surface which is traced out by $z$, when different values are assigned to $x$ and $y$. According to the primary definition, returns are decreasing where the surface is thoroughly concave.[1] The secondary definition is something very different from this; more different than appeared while we were dealing with only one factor. Before, given a point $x$, we took a point below it, $x_0$, and compared the increment of produce due to the dose $(x_1 - x_0)$ with the increment due to the dose $x_2 - x_1$. Now, given a point $(x_1, y_1)$, we have to take a point $(x_0, y_0)$ (where one at least of the variables subscribed 0 is less than the corresponding variable subscribed 1), and to compare the increment of product due to the (compound) dose $(x_1 - x_0, y_1 - y_0)$ with the increment of product due to the dose $(x_2 - x_1, y_2 - y_1)$, where the points $(x_0, y_0)$, $(x_1, y_1)$, $(x_2, y_2)$ are in a right line. For the special case in which the point $(x_0, y_0)$ is at the origin $(x_0 = 0, y_0 = 0)$, the primary and secondary definition come to the same. But this is now a *very* special case. For there are any number of other lines, besides the one passing through the origin, which may be drawn (in the plane of $x$, $y$) through the point under consideration $(x_1, y_1)$. It may well be that, by comparing increments corresponding to points on some line not passing through the origin, the surface may be shown to be *convex* in the neighbourhood of $(x_1, y_1)$, though by the test of the "secondary" sort it appeared concave. Accordingly, I do not hold with the writers who attach a mighty importance to the question whether, if all the factors of production are increased in a certain proportion, say $a : 1$ (where $a$ is greater than 1), the product is, or is not, increased in that proportion. The matter has little to do with that character of the function $z$ with which

function of $\kappa$ as shown in a subsequent note. This function may be represented by the curve in Fig. 1 B (the abscissa now denoting the money-value $\kappa$). The reverse function which has $\kappa$ as the dependent, $Z$ as the independent variable—the cost-curve—may be represented by the curve in Fig. 1 A.

[1] See Index, s.v. *secondary*.

the entrepreneur is, and the economist should be, especially concerned, the fulfilment of the condition of a maximum.

Nor is the breach between the two definitions healed by taking for our lower point $(x_0, y_0)$—not $(0, 0)$, but—$(0, y_1)$ [or $(x_1, 0)$]; and observing whether ($ax_1$ with $y_1$) [or ($x_1$ with $ay_1$)] will produce more or less than $a$ times what ($x_1$ with $y_1$) will produce.[1] According to both these subordinate varieties of the secondary species, as well as the preceding main one, it might appear that the surface at the point $(x_1, y_1)$ was thoroughly concave; the sections of the surface formed by three vertical planes through the point $(x_1, y_1)$ showing each a curve *concave* in that neighbourhood. And yet some other vertical section through the point might show a *convex* curve. Thus, if $x_1$ represent the number of cattle, $y_1$ the number of men attending to them, on a grass farm of given size, it is quite possible that each of the three variations ($ax_1$ with $ay_1$), ($ax_1$ with $y_1$), and ($x_1$ with $ay_1$), compared respectively with $(x_1, y_1)$, might present diminishing returns in such wise that it would not pay the farmer to adopt any of these arrangements. And yet it might pay him to increase one of the factors $a$ times, and the other $\beta$ times, since the increment due to the change, compared, as it should be, with the cost incurred might well show an increasing return (in the primary and here essential sense). In the note will be found[2] an example in which, if $a$

[1] The phrase what $ax$ with $y$ will produce (or what $ax$ with $ay$ will produce), borrowed from Professor Carver (*loc. cit.*), is used as the equivalent of $f(ax, y)$ (or $f(ax, ay)$).

[2] What concerns the entrepreneur is the sign of $\frac{d^2\zeta}{d\kappa^2}$, where $\zeta$ as before is the gross yield in money and $\kappa$ is the cost in money (say $x\pi_1 + y\pi_2$, where $\pi_1$, $\pi_2$ are the—supposed constant—prices of the factors). This comes to the same as $\frac{d^2z}{d\kappa^2}$, where $z$ as before is the product in kind and $\zeta = zp$, if $p$ may be treated as a constant.

For example, suppose that initially the amounts of the two factors are $x = 2$, $y = 1$. And let the law of production, in the neighbourhood of these values, be such that the gross profit $\zeta$ $(= zp) = 9x - 5y - 3x^2 + 4xy - y^2$. Which is also $= z$, the product in kind, if $p$, the price of the product, is unity. Also let the outlay on the factors, $\kappa = x\pi_1 + y\pi_2 = x + y$; the price of each factor being unity.

Initially—when $x = 2$, $y = 1$—the product (and gross profits) are 8. Now compare this with the product of $ax$ with $ay$, where $a$ is $\frac{5}{4}$. That product proves to be 8·25, *less* than $\frac{5}{4} \times 15$. A like result follows for values of $a$ less than $\frac{5}{4}$. Likewise if the product of $ax$ with $y$, or of $x$ with $ay$ is compared with the product of $x$ with $y$, diminishing returns in the proposed sense are shown. And yet the returns are increasing in a sense with which the entrepreneur is principally concerned. For putting $Z$ for the net profits $(= \zeta - \kappa)$, find the locus at which $Z$ is a maximum for any assigned value of $\kappa$. Considering a line $x + y = \kappa$ in the neighbourhood of the initial point (at which $x = 2$, $y = 1$, $\kappa = 3$), find the value of $x$ and $y$ which maximise $Z$, that is, $8x - 6y - 3x^2 + 4xy - y^2$

is $\frac{5}{4}$, $\beta$ is 2, Increasing Return is shown; though if $\alpha = \beta$, or if either of them is 1 (unity), the case seems to be one of Diminishing Return.

A more practical instance could no doubt be attained from the business of railways, or Trusts; assuming (as may often, I think, be legitimately assumed) that the directorate is not deterred from offering additional services to the public by fear of demand falling off.

The divergence which has been indicated between the primary definition of the terms in question and that which is suggested by the semi-mathematical treatment of the subject becomes aggravated when, instead of dual, we have plural factors.

The property of plural factors which has been pointed out, that in starting from any point (system of factors) there is a choice of directions, is connected with the property that in moving from any initial point to the position of maximum, there is a choice of paths.* By the purely mathematical economist the free path would be conceived as movement in that direction by which the greatest increment of profit is continually obtained, the *line of preference* (perpendicular to the line of indifference).[1] But it is not possible, I think, to say *a priori* which of various types best represents the working of the managerial mind.

10. *Relative discontinuity.*—The analysis of different paths, different sequences of steps by which a business may be extended, brings into view the circumstance that one factor of production

Substituting $\kappa - x$ for $y$, we find that the resulting expression in $x$ becomes a maximum when $x = \frac{3}{8}\kappa + \frac{7}{8}$; corresponding to $y = \frac{5}{8}\kappa - \frac{7}{8}$. Substituting these values of $x$ and $y$ in the expression for $\zeta$ we obtain for the required locus $\zeta$ an expression representing a curve convex to the abscissa, $\kappa$ (like the curve in Fig. 1 B up to the point $P$); corresponding to Increasing Return certainly in the primary, and quite possibly also in the secondary sense.

These results are independent of the assumption, which has been made for convenience, that the prices of the product and of the factors of production are each unity. The results depend upon properties of the production-function which do not vary with the price: in particular the *saddle-shaped* character of that surface in the neighbourhood with which we are concerned. The secondary test is so deceptive because there is not fulfilled the condition proper to the primary test, the condition of a maximum:

$$\left(\frac{d^2f}{dx^2}\right)\left(\frac{d^2f}{dy^2}\right) > \left(\frac{d^2f}{dx\,dy}\right)^2.$$

The only assumption made is the constancy of the prices of the factors while the amount of production is varied. If that assumption is not admissible, the significance of the primary definition, the insignificance of the definition in terms of proportionate factors are still demonstrable.

* There are here omitted some passages employed in the original to illustrate mathematically the different courses by which an entrepreneur may vary plural factors of production so as to attain the most advantageous combination.

[1] See *Mathematical Psychics*, p. 22.

often varies discontinuously as compared with another. Two factors do not always move continuously like the minute hand and the hour hand of an ordinary clock. The movement is rather like that of a clock invented by the ingenious R. L. Edgeworth, in which one wheel moves with every swing of the pendulum, while another connected with the (hour) hand of the clock moves *per saltum*, the hand taking a jump every $7\frac{1}{2}$ minutes. Or we may liken the discontinuous variation to a flight of very steep stairs; like the steps in the Great Pyramid, each some four feet high.

Take as an example of ledges surmounted by large steps or jumps, plots of land not divisible below a certain minimum, and for the more finely graduated steps, days' labour applied to a plot of land, as in the example above quoted from Professor Carver.[1] Starting from the zero of outlay, we find (i.) for the first plot the cost in money of producing a certain number of bushels $z$ to be of the form $l$, the fixed rent of the plot, $+ \mu$, an outlay varying with $z$. If, as in the case supposed—the case typical of the industries here contemplated—the land without labour produces nothing, the cost-curve must start as in Fig. 1 A at a point, $L$, on the axis of $OX$, above the origin. It follows that initially Increasing Return in the primary sense must rule; whether or not the curve traced by $x$ — and $\xi$, the money value of $x$ — is concave.

Suppose, now (ii.), that labour and capital has been laid out on the first plot up to the point of maximum profit $R$; and that a second plot of land is then taken on. *Ceteris paribus*, and in particular the price of the product being supposed constant, it may be shown that the outlay of "days' labour" on the second plot will present Increasing Return in the secondary sense. And so on for additional plots. It is more to the purpose, I think, to observe that the outlay on labour and tools for the last plot taken on will present Increasing Return in that primary sense in which the first of the two compared doses is reckoned from the beginning of the outlay on the last plot. This follows by a repetition of the reasoning applied to the first plot.

The case considered in the two preceding paragraphs is very important, so important as often to obtain the title of Increasing Return *par excellence*.[2] It plays a great part in the theory of Railway Rates. It is the *rationale* of the often noticed circumstance that an increase in the gross receipts is apt to be accom-

[1] Above, p. 63.
[2] *E.g.* Hadley, *Economics*, p. 154, *note*.

panied with a more than proportionate increase in net receipts. To establish this and other important conclusions, it would be often, I think, unnecessary to take into consideration, as just now, a *second* jump, or large dose. For instance, when Professor Ripley employs the principle to account for the prosperity of the American railways consequent on the increase of business,[1] he may be understood, I think, as regarding the outlay on the construction and maintenance of the railways as corresponding to our $l$ [in the simpler case above labelled (i.)], and the operating expenses as corresponding to our $\mu$.

As costs corresponding to our $\mu$ *initially*, after the *per saltum* variation of general expenses corresponding to our $l$, afford increasing returns (in a certain sense), so *ultimately*, if $\mu$ is continually increased while $l$ remains constant, diminishing returns must set in. This corresponds to the fact made familiar by Dr. Marshall that when an industry is called for a sudden increase of output, the short-period supply curve is apt to be inclined positively.

Having now defined the law of increasing (or diminishing) return considered as a term or conception,[2] let us go on to consider the propositions into which that term enters.

11. *The laws as propositions.*—One class of propositions connects the terms defined with peculiarities in the incidence of taxation. But these are not the propositions usually understood by the laws now under consideration. Rather the attribute that is connected with the quantitative relation above defined is the cause of that relation. Thus, in the statement above [3] quoted from Dr. Marshall, the character of Diminishing Return is connected with conditions of agriculture. It is objected by some that the causes are too diversified to allow us to speak of a law of return. Let us consider this objection with respect to each of the laws separately.

12. *Law of diminishing return.*—The peculiar significance of this law in agriculture has impressed a high authority, Mr. Bullock,[4] so much that he proposes to restrict the law to "the productivity of labour on a definite tract of land." Of course, now

[1] Report of the United States Industrial Commission, Vol. XIX. *Transportation*, pp. 277, 286–7. Similar expressions as to the nature of Increasing Return might be quoted from other railway experts.

[2] Some authoritative uses of the terms, presenting points not very directly related to railway economics, will be examined in an Appendix (below, p. 95).

[3] Above, p. 62.

[4] *Quarterly Journal of Economics*, Vol. XVI. (1902), p. 484, and context.

that the law is considered as implying a cause, it is quite legitimate to give a narrow interpretation to the always somewhat arbitrarily limited word "cause." Thus, the laws of the tides according to Sir G. Darwin, who recognises that "a true tide can only be adequately defined by reference to the causes which produce it,"[1] presumably relate only to the periodical oscillations of the sea caused by the moon and the sun, but not to those caused by periodic winds, variation of atmospheric pressure, etc. However, it is "practically convenient" to speak of those changes as "meteorological tides."[2] A like use of qualifying adjectives—recommended by Dr. Marshall as suitable to economics—might remove Mr. Bullock's scruples. That they are not obstinate scruples I infer from the author's use of the terms in a work subsequent to that above quoted.[3]

The diversity of cause may appear greater in the case of Diminishing than in that of Increasing Return, if with Professor Seligman we include the Law of Diminishing Utility under that of Diminishing Returns. And certainly it is not easy to keep the two laws quite separate; especially if the former includes increasing disutility, affecting the cost of labour.[4]

On the other hand, there is in one respect a greater unity in the action of Diminishing Return—that it always rules, *provided that we take sufficiently large doses.* In the nature of things the function representing net advantage cannot increase indefinitely as the factors of production are varied; its value must ultimately pass through a point of maximum—a *Wendepunkt.* This circumstance, it should seem, has so impressed one whose impressions deserve attention, that he regards the law of diminishing return as "no more than an axiomatic statement of a universal principle" . . . "an axiomatic and sterile proposition."[5] My criticism of

[1] *The Tides and Kindred Phenomena,* p. 2. [2] *Loc. cit.*

[3] *Elements of Economics* (1905), ch. v. § 44. "What is true of land is true also of labour and capital."

"Beyond this point ["twenty acres which will perhaps yield the largest return obtainable from the labour of one man"] it will not pay to invest land and capital if the services of only a single worker are available; so that we find here diminishing returns to investments of land and capital with a given supply of labour."

[4] I accept Dr. Marshall's distinction: "The tendencies of diminishing utility and diminishing return have their roots, the one in qualities of human nature, the other in the technical conditions of industry" (*Principles,* ed. vi. p. 170, note); and I evade the difficulty that the price of labour (which enters into Net Returns) has roots in the qualities of human nature, by treating the price of labour along with that of other factors of production as a constant—as I think it is allowable when treating of rates and fares, but not wages and salaries.

[5] Wicksteed, *Commonsense of Political Economy,* pp. 529, 530.

this pronouncement may be expressed in terms of a metaphor which I have already employed, the representation of net profits by the height of a mountain-shaped surface above the plain. If an Alpinist, with a view to climbing up to the summit, seeks to ascertain the configuration of the surface of his immediate neighbourhood; what are we to think of a guide who protests that there is no need for anxious inquiry whether the surface is concave or convex, for (as no mountain rises to heaven) a sufficiently long step must always lead downwards; that, therefore, it is not only true but a truism that the surface with which the Alpinist is concerned is *concave* (viewed from below).

With respect to Diminishing Return in the sense which is of practical interest in industry generally, I think we may say that the phenomenon has all manner of causes [1] except or besides those botanical ones which are characteristic of the law in its first and still most important form relating to agriculture.

13. *Law of Increasing Return.*—I shall now attempt to summarise the various conditions which are attended with the attribute Increasing Return in one of the senses above distinguished.

(1) First may be placed the circumstance which Mill places first,[2] that some things in order to be produced at all must be produced on a large scale—a railway, for instance. Here the outlay up to the large minimum requisite to produce any return at all may be considered as producing no return; and accordingly the cost-curve corresponding to *OLPQR* in our Fig. 1 A,[3] starts from the origin as a vertical line, which we have seen involves the character of Increasing Return (at least, in the secondary sense).

(2) Next I would place the general principle that size is favourable to multiplication of parts, and so to " co-operation " in the sense in which the term is employed by J. S. Mill after Wakefield, " organisation," as it is now usual to say, " differentiation-and-integration " in the technical phraseology of Herbert Spencer,[4] " system " in the language of good old Bishop Butler.[5]

[1] Professor Seligman has well illustrated the variety of causes leading to a similar result in different departments of production :—

" If we crowd more people into the same omnibus, or run more trams over the same track, or make the labourer tend more looms, or put more manure into the same field, we have a more intensive utilisation, until finally the intensive margin is reached where the additional returns will not compensate the additional effort or outlay."—(*Principles of Economics*, § 88 and context.)

[2] First among the advantages of the Joint-stock principle, *Political Economy*, Book I. ch. ix. § 2.

[3] Above, p. 64.

[4] Employed by Dr. Marshall.—*Principles*, Book IV. ch. viii. § 1.

[5] Preface to *Sermons;* and *cf.* note to Sermon on the Ignorance of Man.

Mangoldt [1] and F. B. Hermann [2] may be referred to as putting the matter particularly well.

(3) Where there is a co-ordination of several parts or factors, it often occurs that one varies discontinuously as compared with another, in the manner above illustrated.[3] Foremen and the workpeople whom they supervise may be instanced. When of $r$ foremen each has the full complement of workpeople to which he can attend with advantage, if an $(r + 1)$th foreman is taken on, Increasing Return acts before the foreman last taken on has his full complement of men.[4] There occurs the gain described by Babbage and J. S. Mill as employing the workpeople, and likewise the machinery, up to their full capacity. This advantage is what Jevons [5] designated "Multiplication of Services," attributing its first enunciation to Archbishop Whateley. As we have seen, this principle is a main cause of increasing returns in the railway industry.

(4) Next I should place the classical trio of advantages attributed by Adam Smith to Division of Labour. Their importance is diminished indeed, but not abolished by modern conditions. Practice makes engine-drivers, as well as pin-makers, perfect. There is, I suppose, less "sauntering" on the part of porters at a large than at a small station. The invention of distant signalling by a points-man [6] who sought to spare himself trouble may match Adam Smith's example of his third advantage.

[1] *Grundriss*, § 29.

[2] *Staatswirtschaftliche Untersuchungen*, p. 217, ed. 1890. One of Hermann's examples, the extinction of a fire, illustrates training ("eine eigens geübte Mannschaft") as well as system. The latter advantage in its purity may be illustrated by a primitive version of the example. Once upon a time—soon after the invention of fire, perhaps—there was a conflagration to extinguish which a number of men carried buckets full of water from a neighbouring stream to the scene, of the fire. Then supervened an organising intelligence directing the men to stand in a row at a distance of a couple of yards or so from each other and to pass the buckets without moving from their respective places. Thus the labour of a number of men running to and fro (or at least the excess of the original leg-work over the substituted arm-work) was saved by mere organisation: nothing but an idea (and the numbers requisite for its realisation). This kind of Increasing Return is too much ignored by writers on Railway Economics who dwell exclusively on our *third* type (*cf.* ECONOMIC JOURNAL, Vol. XXI. p. 370).

[3] *Loc. cit.*, p. 78 *et seq.*

[4] For a more exact statement see below, subsection 20, on prime cost; where in general for $\pi x$ should be substituted $\mu$, a function of $x$ (as in the parallel passage, *loc. cit.*, p. 369); a function which is zero when $x$, the amount of product due to an additional dose of the discontinuous variable (*e.g.* an additional foreman) is zero, and small when $x$ is small, and so secures the fulfilment of Increasing Return (in a certain sense), initially at least.

[5] *Economic Primer*, p. 35.

[6] Findlay, *Working and Management of an English Railway.*

(5) Follows a large miscellaneous class of advantages more or less cognate to the above, enunciated by a variety of eminent writers, from Plato to Marshall.

To head (1) we may refer an advantage such as that which is enjoyed by large ships in respect of resistance to the water.[1]

May we not refer to the same head the stimulus which the presence of numerous fellow-workers imparts to the performance even of unco-ordinated operations? [2]

Should we connect with head (2) [and head (3)], the Platonic *καιρός*, the power of utilising opportunity attributed to Division of Labour? This advantage may be illustrated by the efficiency of a Fruit-Car Line which lends its services to different railways in districts so diverse that the respective fruit-crops ripen at quite different periods—peaches in Georgia after the middle of June, citrous fruit in California between December and May. The large private line supplies refrigerator cars in sufficient number to transport the whole of each crop in due season. Whereas, if each railway were itself to collect the fruit in its own district, it would not have cars enough to transport the fruit at the seasonable moment (or would have at other times to keep them idle).[3]

May we refer to some of the other heads an advantage which some may think ought to constitute a separate head, and, indeed, the first head? It is, indeed, first historically, as it comes first in Plato's enumeration; and it is not last in importance. This is the giving to each the task for which he is best fitted; classifying the work-people according to their capacity.[4] But may not this advantage be subordinated to our head (2), together with head (3), the advantage of employing differentiated organs to their full capacity?

Shall we refer to the same heads the advantages procured through "integration," when all the processes of production, from the raw material to the finished product, are performed by the same firm—especially when there is advantage not only to the large firm, but to society, by dispensing with "the need of maintaining too many selling agencies"? [5]

With head (3) may we connect, as Jevons does, what he calls the "Multiplication of Copies"? [6] And may we not refer to the

[1] *Principles of Economics*, ed. vi. p. 290.

[2] *Cf.* E. Wakefield (*Ireland*), 1812).—"Irish labourers never work singly . . . the people there have a sympathy of feeling which makes company necessary for those at work."

[3] Johnson and Huebner, *Railway Traffic and Rates*, p. 234.

[4] *Cf.* Mill (*loc. cit.*) quoting Babbage.

[5] Hadley, *Economics*, p. 154.

[6] *Economic Primer*, p. 36.

same head the advantage of Interchangeable Parts, on which Dr. Marshall has particularly dwelt?

I do not attempt a full enumeration, nor do I insist on the logical affiliation which I suggest. I am only concerned to point out that there is a certain thread of connection holding together the majority at least of the advantages which have been enumerated. There is only one cause, I think, which lies apart from the others; and it is one, I think, usually, perhaps properly,[1] not included among the causes of Increasing Return. That is the greater stability of a large business, the connection between magnitude and the principle of insurance.[2] It was *a priori* improbable, and ultimately proved false, that all Antonio's multifarious ventures should have failed:—

> "From Tripoli, from Mexico and England,
> From Lisbon, Barbary and India."

A large railway, serving varieties of industry and pleasure-seeking, is less likely to lose its custom than a small line which depends on one class of custom.

In the railway industry the practical importance of the cause above placed third no doubt deserves the pre-eminence assigned to it by experts; but in a philosophical view of the subject it should be recognised that there are many other causes about as operative in the railway industry as in other departments of production.

I go on to the cognate conception of Joint Cost.

14. *Joint Cost.*—This conception is not only cognate, but even coincident, with that of Increasing Return, according to one of the parties in a battle of giants which has been fought in America over the matter.[3] I agree with Professor Taussig that there are two distinct conceptions; but I concede to Professor Seligman that they have a certain attribute in common, and that the cases which they denote are frequently coincident.

(1) The two terms, as I understand, correspond to two distinct

[1] For the increasing return attends the large scale only provided that the large scale is attended with a plurality of causes that are *independent* in the sense appropriate to the theory of Probabilities, and the proviso sometimes fails (as probably in the case of some Trusts).

A like objection might, however, be made to the attribution of Increasing Return under other heads. For instance, increase of size is not *necessarily* attended with increase of "differentiation" (above, subsection 2).

[2] J. B. Clark was, I believe, among the first to point out the advantage of large concerns in this respect (*Quarterly Journal of Economics*, 1892).

[3] See *Quarterly Journal of Economics*, Vol. XX. p. 631, and Vol. XXI. p. 156; referred to by Mr. Maurice Clark in his *Local Freight Discrimination*, pp. 27, 28.

mathematical conceptions, which may be simply presented as follows:—Let $x$ and $y$ be the respective amount of two joint products [1]; and let $z$ be the cost of production, upon the usual supposition that the factors of production are employed according to the best available methods.[2] Then if $P$ is a point in a plane, say the plane of the paper, of which the co-ordinates are $x$ and $y$, $z$ may be represented by the height of a surface above the point $P$. For example, let the surface be represented by that of a half-orange placed on the plane of $x$, $y$ so that the base of the hemisphere is coincident with the circle $XOYo$ in our Fig. 2. Now through any point $P$ (within the square $BCAO$) hold a knife parallel to the line $OY$ and perpendicular to the plane

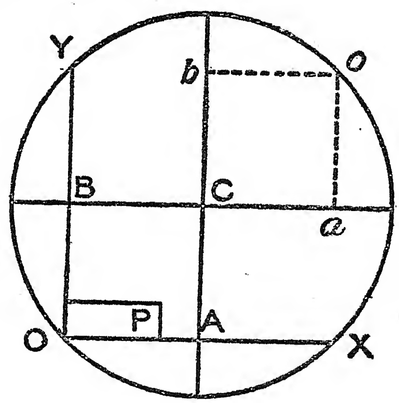


FIG. 2.

of the paper, and cut or suppose cut, a thin slice or section of the orange. The curve which bounds that section will represent the (total) cost of producing *any* value of $y$ together with the given value of $x$, the abscissa of $P$. As the value of $y$ increases (the value of $x$ remaining constant), the corresponding value of $z$ increases,

[1] The reader will observe that these symbols are not used here in the same sense as in former paragraphs (above, p. 65 *et seq.*). The familiar $x$ and $y$ being now reserved for the quantities on which it is now desired to fix attention, the joint products, let us put $u$ and $v$ for the factors of production, at first supposed two in number. Let $x = \phi(u,v)$, $y = \psi(u,v)$; and accordingly $u = f_1(x,y)$, $v = f_2(x,y)$. If, as before, the respective prices of the factors are $\pi_1$, $\pi_2$, we have $\pi_1 f_1(x,y) + \pi_2 f_2(x,y) = z$ (corresponding to $\kappa$ in former paragraphs). If $p_1$, $p_2$ are the respective prices of the products, we have $p_1 x + p_2 y - z$ for the net product which is to be maximised. If $p_1$ and $p_2$ are treated as constant, $z$ must fulfil the conditions of a *minimum*.

If there are more than two factors, the case is to be treated on the analogy of a single product with plural factors of production (above, p. 74 *et seq*). If there is only one factor, the case degrades to the rudimentary type defined by Mill, as mentioned in our text (below, p. 88).

[2] Above, p. 69, note 4.

but at a diminishing rate. For the *slope* of the curve[1] continually decreases as $y$ is increased, it is less for the point (of which the co-ordinates are) $x$, $y + \Delta y$ (where $\Delta y$ is a small increment or "dose" of $Y$) than what it is for the point $x$, $y$. This relation between contiguous points we know to be a condition of Diminishing Cost or Increasing Return. The relation is quite distinct from the relation between the said slope at the point $x$, $y$, and the similarly defined slope at the point $x + \Delta x$, $y$. This latter relation is the criterion of Joint Cost or Joint Production.[2] If, as we change from $x$ to $x + \Delta x$, *ceteris paribus*, the increment of $z$ due to an increment of $y$ becomes smaller, this means that an increase in the production of the commodity represented by the abscissa ($x$) makes it less costly to increase the production of the commodity represented by the ordinate ($y$).[3] In the example given—a hemispherical surface—Increasing Return and Joint Cost go together. But it is easy to imagine a surface—that of a melon, for instance[4] —for which the contrary is true. Even the simple example which we have given suffices to show that the two characteristics are not identical. If $P$ be taken very near $CA$ while far from $CB$, it may be shown,[5] and is perhaps self-evident, that the slope in question will decrease very slightly in consequence of an increase

[1] The "slope" of a curve at any point therefrom is here used to denote the tangent of the angle made with the abscissa by a tangent to the curve at that point. For the curve under consideration (in a plane perpendicular to the plane of the paper) the line through $P$ perpendicular to $OX$ (in the plane of the paper) is to be taken as the abscissa.

[2] I use the two terms to denote (different aspects of) the same phenomenon.

[3] In symbols, $\frac{d^2z}{dx\,dy} < 0$; where differentials do duty for finite differences, increments which may have different magnitudes according to the context and purpose. More exactly the characteristic (of Joint Production) may be written :—$f(x + \Delta x, y + \Delta y) - f(x + \Delta x, y) - f(x, y + \Delta y) + f(x,y) < 0$; where $f(x,y)$ denotes the cost of producing the joint products $x$ and $y$.

[4] See below, p. 89, note 1.

[5] The equation to the surface may be written

$$z = \sqrt{2c^2 - (c - x)^2 - (c - y)^2};$$

where $c$ is put for the radius of the circle divided by $\sqrt{2}$. We have then for $\left(\frac{dz}{dy}\right)$, the differential coefficient of $z$ with respect to $y$ on the supposition that $x$ is treated as a constant, $+ (c - y)/z$, which is positive, provided that $y$ is less than $c$, as postulated in the text. The differential coefficient of this expression with respect to $y$ while $x$ remains constant, say $\left(\frac{d}{dy}\right)\left(\frac{dz}{dy}\right)$, is $\frac{-z^2 - (c - y)^2}{z^3}$; which is always negative and generally considerable. But $\left(\frac{d}{dx}\right)\left(\frac{dz}{dy}\right) = \frac{-(c - x)(c - y)}{z^3}$; which also is always negative, but becomes evanescent as $x$ approaches $c$.

of the abscissa (alone); while as before it decreases sensibly in consequence of an increase of the ordinate (alone).

The converse relations of Diminishing Return and the usually unnamed opposite of Joint Production which I have proposed to call Rival Production [1] may be illustrated by the half-orange, if we reverse its position so that what was before its highest point is now its lowest, the point $C$ at which the surface now touches the plane of the paper. If $C$ is now taken as the origin it will appear that the slope with which we are concerned will *increase* in consequence of an increase of either the abscissa or the ordinate (for any point $p$ within the area $Caob$). Diminishing Return and the opposite of Joint Production go together. Such consilience is quite common. But it is by no means universal. For example, honey and certain fruits are, I believe, joint products; the flowers which produce the fruit being fertilised by the bees which produce the honey. But though Joint Cost thus operates, it is quite possible that an increase of fruit trees *ceteris paribus* would be attended with Diminishing Return. And the increase of hives *ceteris paribus* may have a like result.

These propositions remain true when we remove the clause "*ceteris paribus*," and consider Diminishing Return in its most general and genuine signification as equivalent to the condition or criterion of a *maximum*.[2] The circumstances may be such that in whatever proportions we increase the factors of production, bees and fruit trees, each successive increment of cost will be attended with a less than proportionate increment of produce.

(2) The two conceptions are clearly distinct. But though not coincident they are cognate. There is a certain general resemblance between Increasing Return and Joint Production in so far as both seem to fulfil the dictum: "Unto him that hath shall be added." More exactly, the resemblance may be traced with respect to one or more of three distinct features.

First (*a*), there is a certain correlation between the character of Increasing Return in the proper sense of the term and that of Joint Productivity in the sense above explained. The greater the Productivity the more probable it becomes, other things remaining the same, that the case will be one of Increasing Return. If in the example just now adduced we suppose the stimulus to the creation of honey given by the increase of fruit trees to become indefinitely greater while other features of the case remain

[1] Economic Journal, Vol. VII. p. 54, referring to *Giornale degli Economisti*, 1897. "Disjunctive" might be suggested as the antithesis to "Joint."

[2] See Economic Journal, Vol. XXI. p. 357, p. 364 and context

the same, the case will become ultimately one of Increasing Return.[1]

This is, I think, the most general view of the correlation between the two conceptions. But there are other kinds of consilience which depend upon some particularity in the function which expresses the relation between cost and products.

(*b*) *Pro forma* I begin with the simplest case which Mill begins with: "when the same outlay would have to be incurred for either of the two [products] if the other were not wanted or used at all."[2] Supposing that increase of one commodity $x$ is always attended with the increase of the other in some definite relation,[3] the two characteristics will evidently concur.

A more important case arises when the Joint Cost depends upon a quantity such as total weight or volume which is the sum of two or more items each pertaining to one of the Joint Products.* The cost of carrying gold and silver, for example, might depend only on the avoirdupois weight, in a primitive regime, making abstraction of "general" expenses. There might be a co-operation [4] between native bearers such that an increase of the burdens would not require a proportional increase of men. Increasing Return would then be realised, together with Joint Production. Nor is it necessary to suppose that the contributions of the two articles to their Joint Cost are simply proportional to the respective weights. Differences of specific gravity (affecting the relation of volume to weight) or of value (affecting the amount of insurance) might be relevant. And yet upon probable assumptions, which it

[1] Let $x$ denote the amount of fruit produced, $y$ the amount of honey; and let the total cost of production be $f(x, y)$. In order that Joint Production, in the sense in which the term is here taken, may obtain, $\frac{d^2f}{dx\,dy}$ must be *negative*. In order that Diminishing Return, in the proper "primary" sense of the term, may obtain, we must have not only as indicated in the preceding paragraph of the text

$$(1)\ \frac{d^2f}{dx^2}\ \textit{positive, and}\ (2)\ \frac{d^2f}{dy^2}\ \textit{positive;}$$

but also (3)

$$\frac{d^2f}{dx^2}\frac{d^2f}{dy^2} - \left(\frac{d^2f}{dx\,dy}\right)^2\ \textit{positive.}$$

The last condition will be violated if *ceteris paribus* the value of $\frac{d^2f}{dx\,dy}$ is supposed to increase indefinitely (in absolute magnitude).

[2] *Political Economy*, Book III. chap. xvi. § 4.—Mill begins with this case but he does not end with it. He continues: "In a more partial sense, mutton and wool are an example, beef, hides, and tallow. . . ."

[3] $y = \phi(x)$, while $\frac{d\phi}{dx}$ is continually $+$.

* See Index, s.v. *Joint Production*, for further reference to this case.

[4] Increasing return of our species 2, above, p. 81, unmixed with species 3.

suffices to indicate in a mathematical note,[1] Increasing Return and Joint Production would be consilient.

But this is not the leading case of correlation between the two compared conceptions. That is to be found rather (*c*) in the circumstance that frequently joint products depend upon one and the same factor of the kind above described as varying *discontinuously*, like the amount of land (relatively to the labour employed thereon) in the illustration given. Thus the carriage of a passenger at a time when there is no public service presupposes the running of a special train. Accordingly, the transportation of the passenger and that of luggage (at the specified time) are joint products. For—starting from zero—it is impossible to increase the one kind of transportation without rendering the other less costly.[2] In the same circumstances the transportation of a *second* passenger is attended with decreasing cost. So, considering the general expenses of permanent way and staff, we shall often find that passenger traffic and goods traffic are joint products. The arrangements necessary for a certain [3] increase of one facilitate the increase of the other. The case then is one of Joint Production. But it is also apt to be a case of Increasing Return. For, as shown above,[4] in these circumstances Increasing Return, at least of the *secondary* kind, is apt to be realised.

Conditions of this kind I think are usually presupposed by the able writers who identify Increasing Return and Joint Production. Thus Mr. Bickerdike in the following passage employs the term " scale of production " in a sense which seems to imply the sort of discontinuity which I have all along in view :—

[1] Let the Joint Cost, $z$, $= F(\alpha + \beta)$, where $\alpha = \phi(x)$ and $\beta = \psi(y)$. Let $F'$ be continually $+$, and $F''$ $-$; and let $\phi$ and $\psi$ have the same properties. Then it may be shown that $\frac{d^2F}{dx\,dy}$ is negative, as well as $\frac{d^2F}{dx^2}$ and $\frac{d^2F}{dy^2}$. If $F''$,$\phi''$,$\psi''$, are each $> 0$, *ceteris paribus*, then the opposite of Joint Cost, Disjunctive Production, co-exists with Diminishing Returns in that proper sense which is defined by the *three* conditions (characterising the second term of variation) of a maximum (or minimum). The theorem admits of generalisation in several directions. Thus $z$ may be $= z_1 + z_2 + \ldots$, where each of the subscribed " $z$'s " have the properties before attributed to $z$. Also here, as throughout, what is predicated of two variables is capable of extension to the case of several variables.

This principle accounts for the consilience between Increasing Return and Joint Cost which we observed in the case of our spherical orange. If we had taken a melon, attributing to it the shape of an ellipsoid of revolution, the consilience would no longer hold good; $z$ now involving not only a *sum* of functions, such as $ax^2 + by^2$, but also a *product*, such as $2hxy$.

[2] ECONOMIC JOURNAL, p. 369; and *cf.* p. 368, and p. 367, par. 1.

[3] *Cf.* below, p. 94.

[4] *Loc. cit.*, p. 88.

"I have spoken of the law of increasing returns and of joint costs as the bases of justification for differential prices,[1] but it would be more correct to say a condition of production such that an increased supply of certain articles or services would make easier an increase of supply of other articles or services, of a further increase of the supply of the same articles or services. That is to say, if cost of production of $x_1$, $x_2$, etc., units, either of different commodities or even of the same commodity supplied to different customers, depends not only on $x_1$, $x_2$, etc., but also on the total scale of production $Z$ $(= x_1 + x_2 + \ldots)$, *i.e.*, on $F_1(x_1,Z), \ldots$"

The symbolism proposed by Mr. Bickerdike appears to imply not only the discontinuity which we are now considering, but also the simplicity which was considered in a preceding paragraph. The *addition* of the joint-products, $x_1$, $x_2$, suggest their having in common a measurable attribute, such as weight or volume. The symbols must, however, be interpreted in a somewhat forced sense,[2] so as to apply to joint products like goods and passengers, which cannot well be added together. But the use of a simple symbolism in a loose sense is the more defensible because, however complicated, mathematical expression can hardly cope with the difficulties caused by the element of Time in economics.[3] That is the justification of our attempt to eke out the deficiencies of formal exposition by means of homely metaphor.[4] Even this resource fails us now; as we should require a *fourth* dimension to represent two factors of production and two products.

[1] The circumstances that both Joint Cost and Increasing Return are favourable to Discrimination is regarded by Mr. Maurice Clark (*Local Freight Discrimination*, p 27) as a reason for their identification. It is not a decisive reason in the view of one who does not regard Joint Cost as the fundamental cause of Discrimination (see ECONOMIC JOURNAL, Vol. XX. p. 460; Vol. XXI. p. 148, and sect. ii. [in the sequel] of the present paper).

[2] I have taken the liberty of placing a comma after $x$. I might suggest using a semicolon and writing $F(x_1;\ x_1, x_2 \ldots)$ where the symbols on the right of the semicolon are to be understood as varying discontinuously; *e. g.* by degrees corresponding each to a train-load or other relatively large unit. Thus if $x_1$ denote (the number of) third-class passengers, $x_2$ that of first-class passengers, the $x_1$ on the right together with the $x_2$ may determine the number of (daily) trains on a given railway; while the $x_1$ on the left denotes the number of third-class passengers on a particular train. The function $F$ assigns the law of cost for the "short period" during which it is proper to treat the symbols on the right as constant, while the symbol (or it might be symbols) on the left of the semicolon are varied. [Compare the symbolism proposed, 1908.]

[3] *Cf.* Preface to *Principles of Economics*, "the element of Time which is the centre of the chief difficulty of almost every economic problem."

[4] *Loc. cit.*, p. 78.

It may be worth observing that the factor here described as varying discontinuously is not necessarily, though it is frequently, prior in time to the continuous factors. Suppose the factor to be the irrigation of crops, as practised by the Virgilian husbandman, who some time after sowing the seed admits the fertilising flood (" semine facto " . . . " *Deinde* satis fluvium inducit ").[1] If the operation of " enticing " the river from its channel could be performed only once in a year or other period during which it was open to the husbandman to plant and dig to any extent, then Increasing Return—of the secondary species, up to a point—would be realised. If there are two plots of ground suited to different crops, and the opening of the sluices to irrigate one plot involves the irrigation of the other, then the crops will be joint products.

In most of the examples which have been given it will be apparent that Joint Production (and its contrary) resembles Increasing (and Diminishing) Return in this respect: that as each is characterised by an increment relative to a dose,[2] so the character may vary with the magnitude of the doses contemplated. To predicate Joint Production without this datum would often be unmeaning. Thus passenger traffic and goods traffic may be considered as joint products with respect to variations on a large scale involving the construction of a new track; both kinds of traffic being thereby facilitated. But for a given track, which is already crowded, an increase of one kind of traffic may well render the other kind of traffic more costly.[3] The case may be one of *rival* production. The complexity of the facts with which the railway manager has to deal transcends the nicety of the nomenclature invented by the economist.

15. *Prime Cost.*—One more cognate term, one more class intersecting those which have been defined, remains to be considered. The affinity or partial coincidence of Prime Cost with the preceding categories is, indeed, not very evident if we identify the term with what is sometimes called the " special cost " of a product, meaning that cost [4] which would have been saved if

[1] Georgic I. 106. For a more modern instance, see below, p. 94.

[2] More exactly the increment of a differential coefficient. See as to the significance of size, subsection 3, *loc. cit.*

[3] Mr. Acworth describes instructively the impediment to through traffic on a railway which might result from the stimulation of short-distance coal traffic. *Railways and Traders*, pp. 124, 126.

[4] I might suggest assigning this signification to the term " special " cost, and to " prime " cost the somewhat different signification proposed in the text. No doubt the line of distinction is very fine. For the special cost of the local freights instanced in the text would be a kind of " prime " cost in so far as those

the product had not been produced. For example, if there were proposed a revision of railway rates for local traffic (extending, it may be, to a great many localities—a large part of the system), "in figuring whether the new rates would be good financial policy, the road must charge against the traffic as its 'special cost' every expense that can in any way be causally traced to the local freight traffic. This means that large items of maintenance, interest on cost of rolling-stock, and structures, etc., etc., must be included."[1]

Such computations must certainly be made by the entrepreneur varying the factors-of-production, whether by large or small doses, so as to realise the maximum of profit,[2] the alert business man acting upon the "principle of Substitution."[3] If it appears that the cost which would be saved by the omission of any branch of production is greater than the yield of the product, of course the branch must be discontinued (unless, indeed, the loss can be converted into gain by a readjustment of factors, or—in a regime of monopoly—by a revision of rates).

But Prime Cost is, I think, usually taken in a somewhat different, or more general, sense, which may be exemplified by omitting in the example just given the "large items of maintenance," and taking account only of operating expenses. In general there is to be excluded in the computation of Prime Cost the cost of some factors of production of the kind above described as varying discontinuously. This computation would, of course, be made more readily than the one above described. It might be of some use, though not of as much use as the more difficult computation. If it appears that the prime cost of the product in the sense proposed is greater than its yield, the production must be unprofitable. But the converse is not true; the prime cost might be less than the yield, and yet the production might not be (in the long run) profitable.

Taken in the sense proposed Prime Cost makes its appearance under conditions which we have seen to be favourable to Increasing Return and Joint Production. Prime Cost, Joint Cost, Decreasing Cost, may often be predicated of the same circumstances. The three classes are related to each other as circles so intersecting as to have a portion of area in common.

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freights presuppose the existence of a railway. And, again, prime cost, even though denoting only operating expenses, may be regarded as a kind of special cost, the cost that could be saved *during a short period* (not admitting of complete readjustment) by the discontinuance of the product.

[1] Maurice Clark, *Local Freight Discrimination*, p. 36.

[2] Above, subsection 3 *et passim*.

[3] *Principles of Economics*, ed. 6, p. 355 *et passim sub voce* "Substitution."

For example, suppose that on a train running from the station $X$ to the terminus $P$, a car (*Anglice* truck) is put on to carry oysters of two descriptions, some grown at $X$, others brought to $X$ from $Y$, a place further from $P$ (via $X$) than $X$ is. The case is a familiar one, being used in a classical treatise to illustrate discrimination. Suppose that the shipment of oysters is attended with some expense (not necessarily the same for the two descriptions of goods), an expense varying with the variation in the amount of oysters shipped, while the expenses incident to running the car—for wear and tear, etc.—remain the same, whatever the load. Then that cost of handling may be regarded as *prime cost*. Also the services conferred by transporting oysters of the two descriptions are joint products. For the transportation of oysters of one description (in quantities amounting to a substantial fraction of a car-load) necessitating the use of an additional car, facilitates the transportation of oysters of the other description (in like amounts). If, for instance, starting from the zero of oysters shipped we add an increment—amounting, say, to some three-eighths of a car-load—of oysters grown at $X$, the cost of adding an increment—of like amount—of oysters coming from $Y$ is thereby reduced; it becomes, in fact, only the prime cost of the latter increment. It results from the same circumstances that Increasing Return is exemplified by both kinds of transportation.[1] If, as before, we start with a substantial increment of one kind, then the cost of a second increment of the *same* kind will be only the prime cost of that second increment.[2] So if a train is put on to carry milk as well as passengers, the cost of the wear and tear, etc., of the trucks required for the milk, together with the cost of handling the milk, is prime cost. The carriage of the milk and that of the passengers are joint products. Also each is apt to exemplify Increasing Returns.

The reader will observe here as throughout how the attribution

[1] The Joint Production is of species $b$ (above, p. 88) as well as species $c$, (p. 89).

[2] See the definition of Increasing Return, subsection 4 (above, p. 66 *seq*). If, reverting to our original notation, for the factor or its cost $x_0$ in that passage we put 0 corresponding to zero of oysters shipped, we may put for the cost of transporting oysters amounting to $\frac{3}{8}$ of a carload, $l + \frac{3}{8}\pi$, where $l$ is the fixed cost of running a car, $\pi$ is the cost of handling a full load, and it is assumed for simplicity that the cost of handling any fractional load is simply proportional to the fraction. Then $f(x_1) = \frac{3}{8}$. Similarly, if we put $l + \frac{3}{4}$ for the cost of transporting $x_2$, we have $f(x_2) = \frac{3}{4}$. Thus

$$\frac{f(x_2) - f(x_0)}{f(x_1) - f(x_6)} = \tfrac{3}{4}/\tfrac{3}{8} = 2 > \frac{l + \frac{3}{4}\pi}{l + \frac{3}{8}\pi};$$

which inequation is the characteristic of Increasing Return.

of Increasing Return or of Joint Cost presupposes some datum as to the magnitude of the doses employed and other circumstances of the case. Our propositions would not have held good for very small doses of transportation, say a basketful of oysters, which would not necessitate an additional car, nor for very large doses, on the scale say of three-quarters of a car-load, the superposition of which would have necessitated putting on a second oyster-car. A passenger is an increment of very different significance with respect to Joint Production and Increasing Return according as he requires a special train, or helps to crowd a public carriage. Likewise the meaning of Prime Cost varies according to the context, point of view, and purpose in hand.

As was observed with reference to Increasing Return and Joint Production, so also with respect to Prime Cost, the discontinuous dose which cannot be renewed during a short period is not necessarily administered at the beginning of the period. For example, the cost of sorting letters which are to go by a certain train may be taken as (part of) the prime cost of postage, the cost of the train not being taken into account. Yet the letters may be sorted before the train is run.

But the order of time is not indifferent in cases where "quasi-rent" makes its appearance in this connection. I leave it to him who first discerned the importance and distinguished the properties of "quasi-rent," to explain the relation of this conception to "prime," and its correlative "supplementary," cost. We are here concerned only with the general principle underlying the distinction between quasi-rent and profit. We have to observe how differently human action is affected by an object as it appears in the future, and when it has become a *fait accompli*. Not even Jupiter, as the ancients would have said, plans about the past. As the general in a campaign or battle acts *pro re natâ*, not strictly adhering to a preconceived plan, so Directors who would not have counselled investing in a railway that, as it has turned out, yields little profit over and above operating expenses, may still be well advised now in operating that unprofitable railway, since a little is better than nothing.

# APPENDIX.

## ON SOME VARIANT TERMINOLOGIES.

1. *Professor Carver's Terminology.*—Professor Carver, in his important observations upon Increasing Return,[1] appears to have had in view the species which we have distinguished as *third.* His theory is, I think, specially relevant to the phenomenon here described as "relative discontinuity."[2] This phenomenon appears to be the main ground of the distinction which he draws between the two questions: "What is the best *proportion* in which to combine the various factors? What is the best *size* for the whole business unit?"[3] The distinction is not conspicuous on the hypothesis of perfect continuity proper to the method of variation above labelled $\gamma$.* The distinction appears particularly applicable to the case of discontinuity above labelled $\beta$.*

Discontinuity also may explain the importance attached by Professor Carver to the limit which separates Increasing from Diminishing Return in the *secondary* sense—the point $Q$ in our Fig. 1. The *secondary* sense enters into a certain proposition which, though a mere truism in the simpler cases, becomes significant where there is more than one *maximum*;[4] the proposition, namely, that, for a given or assigned outlay, the total product is greatest when the average product is greatest. The maxim may be illustrated by a problem which has been already noticed. Suppose that in the case cited from Professor Carver the farmer has a limited amount of capital and labour, say 34 days' labour (with team and tools), to apply to plots of land, which for simplicity we may suppose to be rent free. What number of plots will he find it most profitable to cultivate?[5]

[1] *Distribution of Wealth*, ch. ii.

[2] Above, p. 78.

[3] *Op. cit.*, p. 65.

* The method specified above, p. 77. The Greek letters refer to a passage in the ECONOMIC JOURNAL omitted in this Collection.

[4] In the technical sense, distinguished from the *greatest possible*.

[5] Prof Landry's criticism of Prof. Carver in the *Quarterly Journal of Economics*, 1909, calls for notice here so far as it impugns an assumption which we have made throughout: namely, that if $x$ is the amount of commodity produced and $z$ the amount of a factor employed in the production, say $x = f(z)$; then $x$ always increases (or at least never decreases) with the increase of $z$, $f(z + \Delta z) >$ (or $\not<$) $f(z)$ (ECONOMIC JOURNAL, Vol. XXI. p. 351, note 1, *et passim*)—an assumption countenanced by leading theorists, such as Auspitz and Lieben. Consider the diagram used by Flux, ECONOMIC JOURNAL, Vol. XV. p. 278. [See below, referred to in Section VI., Vol. II. p. 326.] Certainly in a case like that which is adduced below, p. 96, note 2, the increase of the factor (land)

The circumstance that on the two-plot system labour and capital would be employed on each of the plots in smaller amounts than would give the largest product per unit naturally raises the suspicion that this arrangement is not the best. The suspicion proves, indeed, not to be true, as we have seen.[1] But it well might have been true even in a regime of monopoly had the data been different;[2] and would be true in a regime of perfect competition.

The same phenomenon of relative discontinuity appears to justify the distinction which Professor Carver has drawn in a passage[3] of which the substance is as follows:—Let $X$ (acres of land) with $Y$ (units of labour and capital) produce $P$ product. Then (1) if $X$ with $aY$ produce more than $aP$ ($a$ greater than unity), we have a case of "increasing returns." But (2) if $aX$ with $aY$ produce more than $aP$, we have "increasing economy of large scale production." The distinction between (1) and (2) is, I think, specially important in the case supposed by Professor Carver in the context, where $X$ (the number of acres of land) varies discontinuously (as compared with the variation of $Y$)—by doses of ten-acre plots. Yet one may doubt whether the cases are so distinct as to deserve quite different names; and, if so, whether the best names have been adopted.

Firstly, the distinction appears to be one of degree or dimension in this respect, that behind $X$ and $Y$ there is often some $Z$, which, though supposed constant in the above statement, may, under other circumstances, become multiplied by $a$.[4] Thus the

is attended with a diminution of the produce; say $x = F(z)$, where $F(z + \Delta z) < F(z)$. But this relation "$F$" is not identical with that which we have designated "$f$." For we assume that the entrepreneur "applies his outlay to the best of his ability" (*loc. cit.*, p. 357 and p. 568). Accordingly $f$ does not coincide with $F$ beyond the point at which $F(z + \Delta z)$ becomes less than $F(z)$, if the farmer knows the facts designated by the relation $x = F(z)$; if the farmer does not know the facts, $f$ does not coincide at all with $F$. We presuppose, of course, common sense on the part of the business man—and of the economist who theorises about business.

[1] Above, p. 78.

[2] For instance, suppose that in order to produce any crop at all there is required a preliminary expenditure of *seventeen* (instead of *two*) days' labour. Then other things remaining the same, every figure in the first column is to be increased by the addition of 15. If the number of available days' labour is now 68, a *maximum* of profit would be afforded by laying out on each of *two* plots 34 days' labour, that is, less than the amount which yields the largest product per plot, now 35 days. But the *greatest possible* profit will be obtained by laying out the whole of the 68 doses on *one* plot.

[3] *Op. cit.*, p. 66.

[4] See above, p. 65, as to the difficulty of using a single simple formula in order to label the diversified relations between Return and Cost; relations which present different characters according to the magnitude of the "doses," the length of the "periods," contemplated.

above statement refers to a single farmer. But if there were several farmers, might not an increase of their numbers, resulting in an improved organisation, lead to a more than proportionate increase of product? And must case (2) be then degraded from "increasing economy of large scale production" to mere "increasing returns"? [1]

Secondly, even if different names are to be given to cases (1) and (2), it may be doubted whether the names proposed are the best. For this nomenclature, as Dr. Marshall has remarked, "would deprive us of an old use of the term which is of great importance; and in which the distribution of the resources of production among different uses is supposed to have been made carefully and well, so far as the knowledge and skill of those engaged in the industry will carry." [2] Accordingly, it is tenable, in the cases above distinguished as (1) and (2), that the terms Increasing or Diminishing Return had better be applied to the *second* case; while the phenomenon defined by Professor Carver in the *first* case as Increasing Return had better be described as failure of the proper proportion [3] between the factors.

2. *Proportions of factors.*—The term "proportion" appears especially suitable to the adjustment of some factors, say $X$, treated as variable, while some other factor, $Y$, is treated as constant—the case of relative discontinuity above illustrated; but unsuitable in general, apart from this incident, when all the factors are conceived as varying continuously—the type of variation which we have labelled $\gamma$.[4] "Proportion" in this latter use seems to mean nothing more than adjustment of factors so as to obtain the greatest net profit; and this idea is much better expressed by the (greatest possible) maximum value of a function of many variables.[5] Accordingly, I see no advantage

[1] Professor Carver himself admits that "the law of the increasing or diminishing economy of large scale production, while sufficiently distinct from that of increasing or diminishing returns to warrant a difference of name, is yet fundamentally very much like it," *op. cit.* p. 91.

[2] *Principles of Economics*, ed. v. p. 320. Dr. Marshall continues: "The older economists applied the law of Diminishing Return in warnings as to the dangers of the growth of a very dense population . . .; and they consistently assumed that the distribution of resources among different uses would be about the best which were at the command of the population in question."

With respect to such distribution of resources between large and small farms I fail to see that anything is gained by Prof. Davenport's novel terms "Law of Advantage and Size" (*Quarterly Journal of Economics*, Vol. XXIII. p. 610).

[3] In accordance with Prof. Carver's use of that term in a passage cited at the beginning of this Appendix.

[4] ECONOMIC JOURNAL, Vol. XXI. p. 367. [See note * to p. 95 above.]

[5] See note 1 above, p. 76. Compare Marshall, *Principles of Economics*, ed. vi. p. 170: "If his business extends he will extend his uses of each requisite

in substituting for "diminishing return" the phrase "disadvantage accruing from any excess or defect in the relative proportions of the factors of production."[1] A similar substitution would be of no avail in the analogous physical problem, to locate the maximum height of a surface. An Alpinist (prevented, suppose, by a fog from seeing beyond his immediate neighbourhood) requires to know whether he is in a cup-shaped cavity—a "convexa vallis" (convex to the plane of the horizon)—or on a dumpling-shaped surface. He requires the conception of "concave" (and its opposite); and nothing would be gained by substituting such a term as the disproportion between the latitude and longitude of any position, meaning at most its remoteness from the summit. Nothing is gained, and something is lost, by using the term "proportion" where the conception of *function* is required. The single symbol "$f$" conveys more to the instructed mind than all the words that have been written about the Proportions of Factors.

3. *Professor Chapman's Terminology.*—Some of the preceding points may be illustrated by reference to the original paper in which Professor S. T. Chapman has discussed the *Remuneration of Employers*[2] in connection with Increasing or Diminishing Return. Assuming a community to consist of $z$ similar establishments each with one employer and $x$ employés; he considers the question whether, if an additional employer be taken on, the consequent increment to the total product is greater or less than the remuneration of the average entrepreneur. He assumes that the population $zx$ is constant. He assumes also, as I understand, that the play of competition will bring[3] about a determinate value of $z$ and $x$. (To fix the ideas, we may suppose that the entrepreneur's remuneration is totally unmixed with rent, so

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of production in *due* proportion; but not, as has sometimes been said, *proportionately*."

[1] "Proportions of Factors," by H. J. Davenport, *Quarterly Journal of Economics*, 1909, Vol. XXIII. pp. 594, 596.

[2] ECONOMIC JOURNAL, Vol. XVI.; [examined in Section VI, $\gamma$ II. p. 331.]—The "Laws of Increasing and Decreasing Return" which are the subject of Prof. Chapman's article in the ECONOMIC JOURNAL, Vol. XVIII. are to be regarded, I think, as *propositions* of which the predicates are terms defined as here (Prof. Chapman professes agreement with our definition, *loc. cit.*, p. 53), and the subjects are terms more *general* than the subjects of the propositions here contemplated. Compare Prof. Chapman's distinction between the "abstract" and "realistic" statement, in his *Outlines of Political Economy* (1911), p. 105 and context.

[3] As to the play of competition in such a case I may refer to my observations on entrepreneurs' profits in ECONOMIC JOURNAL, see Index, "Entrepreneur." It may be as well to remark that the supposition now made in a parenthesis for the sake of illustration is not necessary for the argument.

that it is open to any worker to transform himself into an entrepreneur, the difference of remuneration compensating for the efforts and sacrifice attending the transformation.) Professor Chapman rightly states that the answer to the question put is affirmative or negative according as Increasing or Diminishing Return acts. But the sense in which these terms are to be taken is not, I think, stated with sufficient precision. In my view the only appropriate sense is a certain one of the subordinate varieties which the secondary definition may present, as above shown in the case of plural factors.[1] Professor Chapman's theorem holds good if by Increasing Return [2] it is meant that ($az$ with $x$) produces more than $a$ times the product of ($z$ with $x$). But the theorem does not hold good if by Increasing Return [3] it is meant that ($az$ with $ax$) produces more than $a$ times ($z$ with $x$).*

The *primary* definition is not germane to the question above stated. It will be required if the question is: What is the value of $z$ for which the total product is a maximum? But we may go some way towards answering that question without being able to ascertain the character of the Return in the primary sense; if we make the probable assumption that the product of a firm always increases (in virtue of intensified organisation) with the increase of the number of firms *ceteris paribus*.[4] For then, as $z$ is increased (from the value determined by competition), the product of the community would continually increase, as far at least as the point at which the entrepreneur's remuneration dwindles to zero.

[1] *Loc. cit.*, p. 76.
[2] As p. 524, par. 1 (*op. cit.*) must, I think, be interpreted.
[3] As p. 526 note, last par., may, I think, be interpreted.
* The proof of these statements will be found in the original.
[4] The assumption is that $\left(\frac{df}{dz}\right)$ is continually positive.

## (D)

## USE OF DIFFERENTIAL PRICES IN A REGIME OF COMPETITION

[JUSTIFICATION of this Paper, which appeared in the ECONOMIC JOURNAL for 1911, is to be sought in an earlier contribution to the Journal, now reprinted in the Mathematical Section ($\zeta$). In that article it was argued that differentiation of prices, discriminating between classes of customers whose demands were different, is generally advantageous to both parties in a regime of monopoly; and may well prove so even in a regime of competition. Mr. Bickerdike, with his usual acumen, disputed the latter clause (in the ECONOMIC JOURNAL for 1911); questioning whether in cases where there is " uniformity of charge based on cost of production," under free competition, any system of discriminating prices could be " better all round," more advantageous to both producers and consumers. And his contention is virtually admitted here, upon a certain definition of " cost of production " and a congruent limitation of the methods by which discrimination may be introduced. Consider, for instance, the case put below of two species of seaweed for which the demand is markedly different (one perhaps required as manure, the other for its medicinal qualities). Yet if the capital, manual labour and so forth—all the cost-of-production abstracting the profits of the entrepreneur—are the same per ton for each species, it is good Ricardian economics to argue that in a regime of competition the price of both articles will be the same, the profits of the " capitalists " will be equal. It is the orthodox doctrine and practical truth that the regime of competition is better all round than any mixture of monopoly. A firm consisting of an employer gaining normal profits, employees earning normal wages and so forth, could not with advantage to all parties either raise or lower the price of either article. But it is not necessary so to lump the remuneration of the employer into the cost of production. It is possible—and usual throughout this Collection (see Index, " Entrepreneur ")—to fix attention separately on the motives and action of the entrepreneur (*cp.*

below, p. 104). In the case before us suppose that the entrepreneurs form a combination for the regulation of buying prices, not affecting the cost of labour and of other factors. It will certainly be to their advantage to raise the price; but not necessarily both prices. Likewise it is probable that the customers may gain *in globo* by a departure from the old pair of prices. The "demand-schedule" for the respective articles being so different, it is highly probable that the money-measure of the total "consumers' surplus" will be increased by raising one price and lowering the other to a certain extent. Thus it is in the interest both of the producers and the consumers to move away from the original position. To be sure, the directions in which they respectively want to move are not the same. But they are most probably not *diametrically* opposite. Whence it follows that certain changes of price, some degree of discrimination, will in general be advantageous to both parties. The reasoning will be better understood after a study of the second part of ζ, II. p. 407 *et seq.*

The conclusion might be worded more strongly than now at the end of this paper, if there were no danger of its being taken as other than a *curiosum*. (*Cp.* below as to the bearing of the argument on Socialism.) At the bar of pure theory my defence might be summed up as follows: Whereas it is alleged that outside Monopoly discrimination can be practised with advantage all round only in some peculiar cases, I reply: firstly, the cases in which discrimination advantageous to both producers and consumers can (theoretically) be practised without any change of the existing regime are very common; and secondly, in cases of pure Competition the advantages of discrimination may (theoretically) be secured by a change of the existing system, substituting Combination for Competition.]

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Mr. Bickerdike's criticism is forcible and fairly aimed; yet I hope to show that it is not very damaging. His main contention is thus stated:—

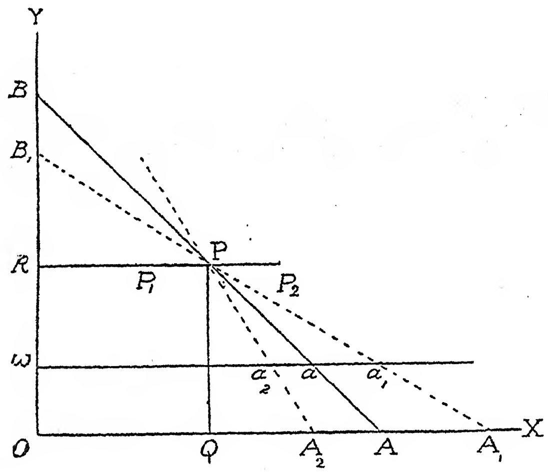
"My argument is that increasing returns, or joint costs, must come in in some way or other if discrimination is more advantageous than uniformity of charge based upon cost of production."

"I question whether any system of discrimination can be better all round than the prices which would be attained under free competition in the absence of any tendency to increasing returns or of joint costs."

"It is difficult to see how any discriminating system would be better for the public."

"It [such a system] could not be socially economical."

In considering the truth and relevancy of this statement, it will be convenient to have before us one of the diagrams [1] employed in the article against a part of which the statement is directed. Let the axis of $x$ in this diagram represent (quantities of) a commodity for which the law of production is not that of increasing returns. The axis of $y$ representing price, let the "demand-curve," for the sake of simplicity, be a straight line. This line is not drawn in the figure, but it may easily be constructed from the line $BA$, which represents *half* the amount of




commodity demanded at any price. Thus there would be demanded at the price $OR$, *twice* the amount $RP$, and at the price $O\omega$, *twice* the amount $\omega a$. To consider discrimination of prices, let us suppose that the class of commodity breaks up into species which differ in respect to the demand of the customers, but not in respect of cost to the producers; for instance, equal hauls of goods, equal in weight, bulk, and facility of handling, and all other circumstances affecting cost, but differing in the value which they acquire by transportation. Let the *dotted* lines form each the demand-curve for one of the differentiated species. Then the *average* of the amounts of the two species demanded at any one-and-the-same price (*e. g.*, $O\omega$) is represented by the corresponding point on the line $BPA$ (*e. g.*, the point $a$, since $\frac{1}{2}$ $(Oa_1 + Oa_2)$

[1] Fig. 2, Economic Journal, Vol. XX. p. 447.

$= O\alpha$). Beginning with the case in which the cost is constant, let us suppose the constant cost to be $O\omega$. If then the uniform charge based on cost of production is $O\omega$, Mr. Bickerdike's statement, as I understand, imports that this unitary price cannot be replaced by a system of different prices for the different species; with advantage to all concerned, both producers and consumers.

The truth of this statement may be shown by observing that if the customers are to benefit by discrimination, one at least of the prices must be lowered below $O\omega$. Suppose, then, that the price of one of them, *e. g.*, that for which the demand-curve is $B_1PA_1$, is lowered from $O\omega$ to $O\omega'$, $\omega'$ being a point on the axis below $\omega$, not shown in the figure. And let the intersection of a horizontal drawn through $\omega'$ with the demand-curve $B_1\,PA_1$ be $\alpha_1'$ a point on that line below $\alpha_1$, the point $\alpha_1'$ as well as $\omega'$ being left to the imagination of the reader. The gain in Consumers' Surplus is then measured by the area of the quadrilateral $\omega\alpha_1\alpha_1'\omega'$. But the loss of Producers' Surplus is measured by the larger area of a rectangle which includes that quadrilateral, namely, the rectangle (not completed in the diagram) of which one side is $\omega\omega'$ and another side $\omega'\alpha_1'$. Likewise if the price is raised above $O\omega$ the consumers lose more than the producers gain. Therefore, the public as a whole, producers *plus* consumers, are losers. *A fortiori*, if the cost is not constant, but increasing (in accordance with the law of decreasing returns).

Mr. Bickerdike's proposition is true, and it may be added, with reference to State regulation of what M. Colson calls "public works," important. But is it contradicted by the proposition which Mr. Bickerdike impugns? The answer is *primâ facie* affirmative. Mr. Bickerdike has accurately quoted the passage in which it is enunciated that "the gain to consumers [through monopolistic discrimination] may well be so great that they are better off than they would have been, other things being equal, under a regime of competition." He has rightly understood that the proof primarily applied to the case in which there is no cost of production is meant to be extended to the general case of substantial cost. He is right, too, in conceiving my thesis to imply that in the case supposed the producers as well as the consumers would be better off than under the regime of competition. He has placed a very natural interpretation upon the passages which he criticises. It would have required a degree of intellectual sympathy beyond what can be fairly expected in a critic to have thought of the explanation which I proceed to offer.

The interpretation of the impugned thesis turns upon the definition of two terms, one of which has received different definitions from classical writers, while the other has, perhaps, not generally been used in any definite sense. These terms are: (1) "cost-of-production," and (2) that which is predicated of the customers of the discriminating monopolist when it is said that "they are better off than they would have been, other things being equal, under a regime of competition." J. S. Mill sometimes employs the term "cost of production" to denote the outlay of the capitalist-employer—the Ricardian "capitalist"—on labour; exclusive of "the reward of abstinence." [1] What if our $O\omega$ represent *this* kind of cost of production! Then the selling price would be well above the point $\omega$; and there would be room for that drop of (one) price, which the fulfilment of the thesis requires. No doubt we ought to include among the "other things that are equal" in the monopolistic and competitive regime payments of interest made to lenders who take no risk. But we cannot suppose the remuneration of the entrepreneur proper to be among those equal things. If a set of entrepreneurs form by combination a monopoly, it is to be supposed that their gains are replaced by what is called in the article under consideration monopoly profit. Likewise, in the converse change, monopoly profits are replaced by entrepreneurs' gains. The preconception that these gains were substantial was naturally present to one who has consistently maintained [2] that the remuneration of the entrepreneur is not to be equated to zero. I am aware that from the point of view of one who surveys all time, the "quasi-rents" enjoyed by the entrepreneur appear as the reward of work and waiting. And if the true rents tend to be evanescent with the progress of freedom and education,[3] what remains of gain proper to the entrepreneur may be so minute and invisible as not to be

[1] J. S. Mill, *Pol. Econ.*, Book III. ch. i. § 1.—"The cost of production, together with the ordinary profit, may therefore be called the *necessary* price or value of all things made by labour and capital."

*Ibid.*, "unless that value is sufficient to repay the Cost of Production and to afford, besides, the ordinary expectation of profit the commodity will not continue to be produced."

*Ibid.*, par. 3, "the outlay [of the producing capitalist] that is the cost of production."

In a later passage, Book III. ch. iv. § 4, par. 1, Mill includes profits in cost of production. In the following section, par. 1, he hesitates between the two definitions.

[2] Most recently in *Scientia* (Rivista di Scienza), Vol. VII. Ann. iv. (1910), pp. 92–94; a passage which may be referred to for further elucidation of the idea expressed in the here following paragraph.

[3] As Mangoldt's *Unternehmergewinn* seems to suggest.

worth disputing about. But it may be questioned whether we have yet approached this limit. There is weight in some observations which Professor Lehfeldt has recently made on this subject [1] :—

"Modern writers on economics have been inclined to describe a class of entrepreneur, who is head of a business and yet buys capital as he buys labour and materials. Now this type, though an important one to describe, is hardly to be found pure . . . financiers' profits are to be classed with true rents."

Thus the conception of a surplus normally accruing to the entrepreneur is not altogether untenable.

But, indeed, I was not thinking specially of perfectly normal competition, but of competition in a more general sense, as opposed to monopoly—not so much "industrial competition" as defined by Cairnes, as that "commercial" competition which he conceives to act among the members of what he calls "non-competing groups." * The profits accruing to members of such groups may be described in the phrase of Mr. J. A. Hobson as "forced gains."

As a type of this case, imagine an island on the shores of which seaweed of rare quality is periodically deposited by the unlaborious sea. The inhabitants, each owning a strip of the coast, exchange seaweed for foreign goods. Competing against each other in what may be called a perfect market, they set up a uniform rate at so much per ton of weed. Now let the competing islanders form a monopoly by combination; and let the monopolistic Directory discriminate between two species of weed, before sold indiscriminately. There is apt to result benefit all round, to both producers and consumers, as compared with the original competitive regime.†

The existence of "forced gains" may properly be postulated with reference to the controversial passage about Socialism which Mr. Bickerdike has quoted. This is a dialectical rejoinder to the

[1] Economic Journal, Vol. XX. (1910), p. 554 and p. 558, and contexts.

* I was thinking also of another species of imperfect competition, or partial monopoly, "industrial" without "commercial" competition; as in the case of a hotel where, with respect to many articles, *e. g.* a cup of tea, bread-and-butter, cake, the hotel-keeper enjoys the characteristic attribute of monopoly, the power of fixing prices; and yet it is open to anyone to become an hotel-keeper (see II. p. 97).

† That is, supposing the seller of sea-weed to possess the character of an entrepreneur in a degree sufficient to render the reasoning on p. 101 (par. 1) applicable. For instance, he might have to lay out money on the purchase of implements or the hire of common labour; which outlay plus compensation for his trouble would be more than covered by the price of the seaweed.

individualistic argument that under a Socialist regime values and the distribution of goods would be much the same as at present. Now, those against whom this argument was directed mostly believe in the prevalence of "forced gains" in the present system. Accordingly, the rejoinder would not be convincingly rebutted by a contention which presumes the absence of such gains.

But, indeed, the rejoinder (to the individualist argument) is more than an *argumentum ad hominem*. The rejoinder does not depend altogether on the proposition disputed by Mr. Bickerdike—that there may be beneficial discrimination in the absence of joint cost or increasing returns. For the character of increasing returns is present wherever there are "supplementary" or general, as distinguished from "prime" or special costs; that is very generally in the modern industrial world. Whether we consider the establishment (and education) of lawyers, or the plant of railways and waterworks, "there is, up to a point, increasing returns," as well remarked by Mr. Bickerdike; and accordingly relative value is apt to be altered by discrimination. It is true that some discrimination may occur in a regime of competition. Our islanders before their combination might possibly have hit upon the plan of selling the two species of seaweed at different prices. Mr. Acworth has adduced some remarkable instances of differential prices occurring in the present regime.[1] But it will be admitted that such discrimination occurs more readily and effectively under a regime of monopoly. It is conceivable, then, that a Socialist Directory should have an advantage in this respect over individualist competition.

Altogether, whatever dialectical value may belong to the passage about Socialism is not much affected by Mr. Bickerdike's observations. Perhaps they were not intended to bear on this passage.

As he has remarked, the main part of my arguments has reference to the comparative advantages of discrimination and uniformity when production is monopolised. I have attempted to investigate some of the conditions on which this comparative advantage depends. With reference to the method of that investigation, I am glad to have the present opportunity of again acknowledging obligation to Mr. Bickerdike's article on Incipient Taxation.[2] In the course of the investigation it appears that

[1] *Railway Economics*, ch. ix.—As to discrimination in a regime of competition, see the article criticised by Mr. Bickerdike and here all along referred to; ζ II. p. 407 *et seq.*

[2] ECONOMIC JOURNAL, 1907, p. 101; *cp.* ECONOMIC JOURNAL, 1908, p. 399 *et seq.* 1910, *loc. cit.*

the comparative advantage does not rest so fundamentally, as sometimes conceived, on the principle of joint cost or increasing returns. For example, suppose the State to own and work two distinct railways or canals, similar as touching their cost, but differing in respect of the demand for transportation. Probably in such a case the State might prescribe a different scale of rates on two lines, with benefit to the public as a whole. The benefit need not depend at all on joint cost or increasing returns. We might suppose, for the sake of illustration, natural waterways for which the costs of construction and all *general* expenses, involving the possibility of increasing returns, are negligible. The benefit depends on a quite different principle, the avoidance of that *perte sêche*, in M. Colson's phrase, that loss of Consumers' Surplus which is incident to a unitary price. That the benefit may be measured by comparison with the state of the customers as it " would have been, other things being equal, under a regime of competition "—whatever that may mean in the case supposed —is at best a secondary proposition.[1] It is an *obiter dictum* not worth disputing about, but for its accidental connection with more important topics to which attention has been called by Mr. Bickerdike.

[1] So described, ECONOMIC JOURNAL, Vol. XX. p. 448, par. 2.

# SECTION II
## THEORY OF MONOPOLY

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### (E)

### THE PURE THEORY OF MONOPOLY

[THIS is a translation of *Teoria Pura del Monopolio* published the *Giornale degli Economisti*, 1897; itself a translation from an English original which has been lost. Much of the contents might with equal propriety have appeared in the Sections dealing with Taxation and Mathematical Economics. But it has not seemed advisable to break up the article. The theory of monopoly in the ordinary sense of the term is connected with the theory of two-sided monopoly or "duopoly." Cournot had represented the transactions between two parties to be determinate in the same sense as competitive prices. But heavy blows had been dealt on this part of his system by Bertrand in the *Journal des Savants*, 1883, and by Marshall, in an early edition of his *Principles of Economics*. Still in 1897 much of Cournot's construction remained standing; the large part which is based on the supposition that the monopolist's expenses of production obey the law of diminishing returns. Now the demolition of Cournot's theory is generally accepted. Professor Amoroso is singular in his fidelity to Cournot (*cp.* ECONOMIC JOURNAL, September 1922).]

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CONTENTS

SECTION I.—The theory of taxation in the simpler cases of a single monopolist dealing with a group (or groups) of competitive individuals.

SECTION II.—Proof of the proposition that in the case of two or more monopolists dealing with competitive groups, economic equilibrium is indeterminate.

SECTION III.—On the effects of taxation (and other kinds of governmental regulations) in the more complicated cases of a

single monopolist dealing with groups of competitors—cases of correlation in respect of production or consumption.

SECTION IV.—Summary in simple language of the theses maintained in the preceding sections.

SECTION I.—*On the effects of a tax in the simple case of a single monopolist dealing with a group (or groups) of individuals competing against each other.*

Cournot has fully discussed the typical case in which a commodity of uniform quality is offered at one and the same price by a monopolist producer to consumers who compete against each other. The price is determined by the condition that the net gain of the monopolist should be a maximum. The quantity which is to be maximised may be represented by the expression

$$pD - \varphi(D), \text{ where } D = F(p);$$

if $p$ is the price, $F(p)$ the quantity of the article which is demanded at the price $p$, and $\varphi(D)$ the cost of producing the quantity $D$. This formula remains applicable if we suppose that $\varphi(D)$ indicates not merely the money cost, the expenses of the monopolist, but the measure of "real cost,"[1] the pecuniary equivalent of the efforts and sacrifices incurred by him in the production. Thus interpreted the formula may be extended, by simply changing the signs, to a monopolist consumer who deals with producers competing against each other. In this case $F(p)$ expresses the quantity of an article offered by competitive producers at the price $p$; and $\varphi(D)$ represents the total utility for the monopolist of the quantity $(D)$.[2]

The effects on the price and on the quantity of an article which are caused by a tax are represented by the same expression in both the cases. If $V$ is the total net utility of the monopolist, whether he is producer or consumer, then for the increment of price consequent on a small tax, for instance, $u$ per unit of product, we have in both cases

$$uF'(p) \div \frac{d^2V}{dp^2};\,[3]$$

which expression is necessarily positive. To investigate the effect

[1] *Cp.* Marshall, *Principles of Economics, sub voce* "Real Cost."

[2] The total utility *simpliciter*, if the monopoly is enjoyed by an individual; but if the part of monopolist is played by a combination—for instance, a co-operative buyers' association—there should be understood the sum of the total utilities obtained by each member from the portion of commodity assigned to him; a conception which is not necessarily identical with the *Gemeinnutzen* of Auspitz and Lieben, relating to a regime of Competition.

[3] Cournot, *Recherches*, Art. 38.

of the tax on the quantity of the commodity taken at the price, it is convenient to consider the price as a function of the quantity; say $p = f(x)$.[1] * Then, if the monopolist is the producer, we have

$$V = xf(x) - \varphi(x),$$

and for the increment of $x$

$$\Delta x = u \div \frac{d^2 V}{dx^2};$$

which expression is necessarily negative. If the monopolist is the buyer, the signs in the expression for $V$ are changed; while the equation for $\Delta x$ remains the same.

As the tax may very well be imposed not on the monopolist but on the competitive group, especially when the latter act as producers, it may be well to observe that in general it makes no difference theoretically on which of the two parties the tax is imposed.[2]

Analogous propositions may be proved for an *ad valorem* tax which is not regressive by inserting in the expression for the tax, instead of $u$, $x$ as just now, any function of $x$ (or of $p$) which increases (or diminishes) with the increase (or decrease) of $x$.[3]

I hasten to pass on to less beaten ground.

An interesting variety of the case in which the monopolist is the buyer occurs when the quantity of the commodity that is on sale is absolutely limited; for instance, when it consists of land offered by competing owners. Here $F'(p)$ is zero, and accordingly

$$dV, = d(\varphi\zeta F(p)) - pF(p), = -F(p)dp.$$

Whence, as the price is continually reduced, the net profit of the monopolist continually increases up to the point at which the sellers are beaten down to nothing—theoretically nothing, practically next to nothing.

In a case of this kind a tax on rent would not fall on the competing landlords at all, but altogether on the monopolist tenant. There occurs in this case what is erroneously supposed to occur in general, that in the phrase of Mill "the price cannot be further raised to compensate for the tax, and it must be paid from the monopoly profits."[4] †

[1] *Op. cit.* Art. 43, p. 89.

* $x$ has been substituted for Cournot's $D$ here and in the sequel.

[2] *Op. cit.* Art. 37.

[3] Cournot, *op. cit.* Art. 41. Marshall, *Principles*, 3rd ed., p. 433, note.

[4] Mill, *Political Economy*, Book V. ch. iv. § 6. He is followed by some eminent writers, but naturally not by any of the mathematical school. See Cournot, *Recherches*, ch. vi., and Marshall, *Principles*, Book V. ch. xiii. ed. 3.

† It may be recalled, however, that, though the monopolist has an interest

It may now be asked: Will the case be materially altered if, between the monopolist buyer and the group that is under the necessity of selling without a reserved price, there is interposed a third party, namely, another competitive group with an ordinary degree of "elasticity." Where there are two groups each consisting of individuals competing against each other the introduction of a third group completely changes the incidence of a tax. Thus a tax on the ground rent of cultivable land will in general fall entirely on the owner; a tax on agricultural produce will not in general fall entirely on the owner. Does there exist a similar distinction in the case of monopoly?

It will be well to begin with the case in which all the individuals in each group are competitors; as the classical writers have hardly discussed this case in all its generality, having limited it by the special supposition that the commodity of which the supply is fixed is not all of the same quality.[1] Let us start with the supposition of three islands, A, B, C, which carry on an international trade of the following description. A buys from B goods, say $b$, for the production of which B must buy from C certain materials or "agents of production," say $c$; which are periodically supplied to C in constant quantities not capable of being increased by human effort—for instance, seaweed deposited on the shores of C. Let $p_1$ be the price of $b$, and $p_2$ that of $c$. Considering any particular producer in B, let us denote by $z_t$ the quantity of finished goods offered by him to inhabitants of A; and by $\zeta_t$ the quantity of raw material or agent of production demanded by him from inhabitants of C. Then the net advantage of this individual, say $u_t$, increases with the net profit $z_tp_1 - \zeta_tp_2$; *ceteris paribus*, and abstraction being made of the efforts and sacrifices involved in the increase of production. Likewise the advantage diminishes with the increase of $z_t$ and increases with the increase of $\zeta_t$ (the increase of material facilitating production) in virtue of these efforts and sacrifices; abstraction being made of the satisfaction resulting from increased gain. These relations may be thus expressed:—

$$u_t = F_t(+(z_tp_1 - \zeta_tp_2), -z_t, +\zeta_t).$$

As $z_t$ and $\zeta_t$ are both controlled by the individual, he will vary

in reducing the tax, it is not a very great interest, for a reason pointed out below. [*Cp.* ECONOMIC JOURNAL, 1922, p. 439.]

[1] Ricardo in his discussion of taxes on raw material introduces at the beginning a phrase applicable to the general case, "that capital which pays no rent" (*Political Economy*, ch. ix. par. 1). But he immediately proceeds to suppose land of different qualities. Cp. Mill, *Political Economy*, Book V. ch. iv. § 3.

them up to the point at which $u_t$ is a maximum. We have thus the two equations :—

$$(a)\ \frac{du_t}{dz_t} = 0 \quad (b)\ \frac{du_t}{d\zeta_t} = 0.$$

Eliminating $\zeta_t$ from the equations (*a*) and (*b*), we might obtain an equation of the form $z_t = \varphi_t(p_1, p_2)$, representing the offer of *b* by the individual No. *t* in *B* (at the prices $p_1$ and $p_2$). Summing the offers of all the producers in B, we have

$$Sz = S\varphi(p_1, p_2) = \text{say } \Phi(p_1, p_2).$$

This offer ought to be equal to the demand in A for *b* at the price $p_1$; say (1) $\Phi(p_1, p_2) = F(p_1)$. Again, eliminating $z_t$ from the equations (*a*) and (*b*) we might obtain an equation of the form $\zeta_t = \psi(p_1, p_2)$. Whence as the sum of the $\zeta$'s is constant we have an equation of the form

$$(2)\ \Psi(p_1\ p_2) = K;\ \text{where } K \text{ is a constant.}$$

To investigate the effect of a tax, say of *u* per unit of seaweed, or use of land or other limited commodity obtained from C, it is proper to put $(p_2 + u)$ for $p_2$ in the equations (1) and (2) from which the two prices are determined. It is evident that the value of $p_1$ which is obtained by eliminating the other variable is not altered by the change; the tax falls entirely on the inhabitants of C.

To study the effect of a like tax on the produce of B we ought to substitute for $p_1$, $(p_1 + u)$ in the left-hand member of the equation (1), or $(p_1 - u)$ in the right-hand member. In general the offer of *b* expressed by $\Phi$ will fall; and consequently the demand for *c* expressed by $\Psi$. The quantity of *c* being fixed, the fall on the demand for it is attended with a fall in its price. There is a limiting case in which the price of *c* is not altered, and the entire tax falls upon A. This occurs when the demand on the part of A for *b* is perfectly inelastic. Then $F(p_1 + u) = F(p_1)$. And so the price paid to the producers in B and their demand for *c* remain unaltered. Everything goes on as before, except that the inhabitants of A pay the tax in addition to the price of *b*.[1]

We have now to consider how these relations are modified when it is supposed that *b* is bought by a monopolist. Equation (2) remains as before; but for equation (1) we ought to

[1] *Cp.* II. 134.

substitute the condition that $V$, the net advantage of the monopolist, should be a maximum; and $V$ is of the form

$$\Theta[\Phi(p_1, p_2)] - p_1\Phi(p_1, p_2).$$

Whence it follows that we ought to equate to zero the differential coefficient of the expression $V - \lambda(\Psi - K)$ (where $\lambda$ is the undetermined multiplier proper to problems of relative maximum) —the complete differential, not simply the partial differential with respect to the variable which is directly under the control of the monopolist, viz. $p_1$. For why should the monopolist stop at the value of $p_1$ which is given by the equation $\left(\frac{dV}{dp_1}\right) = 0$; $p_2$ not varying. He will go on making $p_1$ to vary directly and $p_2$ in virtue of equation (2), indirectly up to the point at which $V$ cannot be increased by any variation of $p_1$ consistent with equation (2).

It appears from this analysis that, as before, a tax on $c$ will fall entirely on C. With respect to a tax on $b$, the case of monopoly agrees with that of competition in this respect, that in general the price of $c$ will be somewhat reduced.

Suppose now that either of the groups B or C becomes solidified as a monopolist. Presumably each monopolist will fix the price which is directly under his control at that figure which he thinks likely to afford him the greatest net advantage, account being had of the price which will probably be fixed by the other monopolist for the article under his control. It is thus that the stroke of a fencer is influenced by his prevision of what his adversary's parry will be. The economic fencing-match may continue till one of the fencers is ruined. Pure theory does not seem to assign any stage at which they must stop.

This is a particular case of the general proposition that, when more than one monopolist takes part in a system of bargains, value is indeterminate. The proof of this proposition presents a difficulty which must be overcome before we can proceed to the more complicated cases of value in a regime of monopoly.

Section II.—*Proof of the proposition that when two or more monopolists are dealing with competitive groups, economic equilibrium is indeterminate.*

To establish this proposition it will suffice to consider the typical cases formed by two monopolists, each of whom, acting independently, offers to a competitive group one of two articles

that are either (A) *rival* or (B) *complementary* as objects of demand.[1]

A. The simplest case under this head is that in which the rival articles are not merely substitutes for each other, but actually identical. This case is treated by Cournot [2] as the first step in the transition from monopoly to perfect competition. He concludes that a determinate proposition of equilibrium defined by certain quantities of the articles will be reached. Cournot's conclusion has been shown to be erroneous by Bertrand [3] for the

[1] I define these terms as follows. I assume, notwithstanding the objections raised by some distinguished economists, in particular Prof. V. Pareto in the *Giornale degli Economisti* [*cp. Manuel*], and Prof. Irving Fisher in his *Mathematical Investigations*, p. 89, that for every system of quantities assigned to the two articles, that is, for every pair of $x$ and $y$ (at any rate for values above a certain minimum of these commodities—*cp.* Marshall, *Principles*, Appendix, Note vi, and passage there referred to), there is for each individual a money measure of the total utility which he derives from the consumption of assigned quantities ($x$ and $y$), a measure represented by a function of those quantities (see Dupuit, article "Utility," *Journal des Economistes*, 1853).

If $x$ and $y$ are the quantities sold at the prices $\xi$ and $\eta$, we have $\xi = \frac{dF_r}{dx_r}$, $\eta = \frac{dF_r}{dy_r}$ for each individual; $x_r$ and $y_r$ being the quantities purchased by the individual numbered $r$. Whence, if $F(x, y)$ is put for $\Sigma F_r(x_r, y_r)$—corresponding to the "Gesammtnützlichkeit" of Messrs. Auspitz and Lieben—$\xi = \frac{dF}{dx}$, $\eta = \frac{dF}{dy}$.

Well then, the articles are rival or complementary objects of demand according as $\frac{d^2F}{dxdy}$ is negative or positive. We shall have the first case when $\frac{d^2F_r}{dx_rdy_r}$ is negative for every individual (or at least on an average); the second case when that expression is positive.

From the last two paragraphs we deduce that $\frac{d\xi}{dy} = \frac{d\eta}{dx}$ is negative for rival and positive for complementary articles. Also, if $x$ and $y$ are considered as functions of $\xi$ and $\eta$ which may be obtained from the above given values of $\xi$ and $\eta$ in terms of $x$ and $y$, it will be found that $\frac{dx}{d\eta}$ and $\frac{dy}{d\xi}$ are positive for rival and negative for complementary articles. The proof of this proposition involves the condition

$$\left(\frac{d\xi}{dx}\right)\left(\frac{d\eta}{dy}\right) - \left(\frac{d\xi}{dy}\right)\left(\frac{d\eta}{dx}\right) > 0.$$

This condition follows from the condition that in equilibrium the total utility of each individual ought to be a maximum because otherwise he will continue to buy at the prices $\xi$ and $\eta$. Whence it is deducible that the total utility $F(xy)$ ought to be a maximum. Whence

$$\left(\frac{d^2F}{dx^2}\right)\left(\frac{d^2F}{dy^2}\right) - \left(\frac{d^2F}{dx,\, dy}\right)^2 > 0,$$

which is identical with the said condition (*cp.* below, Sect. III.).

[2] *Op. cit.* ch. vii.

[3] *Journal des Savants*, 1883.

case in which there is no cost of production; by Professor Marshall[1] for the case in which the cost follows the law of increasing returns; and by the present writer[2] for the case in which the cost follows the law of diminishing returns.

In the last case there will be an indeterminate tract through which the index of value will oscillate, or rather will vibrate irregularly for an indefinite length of time. There will never be reached that determinate position of equilibrium which is characteristic of perfect competition defined by the condition that no individual in any group, whether of buyers or sellers, can make a new contract with individuals in other groups, such that all the re-contracting parties should be better off than they were under the preceding system of contracts.

The theory may be illustrated by the extreme case of decreasing returns,* the case in which there is a fixed limit to the amount that can be produced. Suppose, for instance, that there are two monopolists, each owning a spring of mineral water (Cournot's " source minérale "), the output of which per day is limited to a certain quantity, the same for both springs. To further simplify the example, suppose that the delivery of the commodity is not attended with any expense. Further, let the demand-curve be the same for every consumer; and that the simplest possible, namely, a right line. Thus let $x_r = 1 - p$ where $p$ is the price and $x_r$ is the amount of the commodity demanded at that price by any individual. Accordingly, if $x$ is the collective demand of a set of customers numbering $n$,

$$x = n(1 - p).$$

In Fig. 1 let us represent $x$ by a horizontal abscissa, and $p$ by a vertical ordinate, in accordance with Marshall's well-known construction. We may begin with the supposition that each monopolist deals with only half the total number of potential customers; which is, say, $2N$. The collective demand-curve for one of the

[1] *Principles of Economics*, first ed., note to p. 485.

[2] *Mathematical Psychics*. The competition of the two monopolists will reduce the price below the point $Q$ in the figure on p. 114 (*op. cit.*) to within the tract between $Q$ and $T$. *Cp.* note to p. 116, where the statement that " the system will reach a final settlement at some intermediate point " is inaccurate. Suppose that there are two B's dealing with an indefinite number of A's, as in the case now under consideration. The B's will force each other below the point $Q$; and between that point and $T$ the position of (temporary) equilibrium will continue to vary; since it will always be the interest of one or more of the A's to re-contract with one or both of the B's; getting on to the partial or " supplementary contract curves " which are indicated at p. 37 (*op. cit.*), but not represented in the figure on p. 114.

* For an example not thus limited see Economic Journal, September 1922.

markets thus constituted may be represented by the right line $RC$, making an angle of forty-five degrees with each of the co-ordinates, if the units pertaining to the co-ordinates are properly taken. Let $OR$ be the unit of price. Let $OA$ represent the quantity of commodity which is demanded at the price $OP$ $(= \frac{1}{2})$, $= \frac{1}{2}c$ (units of commodity). Let $OB$, $= \frac{3}{4}c$, be the amount to which the daily output of the monopolist is limited. Let $RC'$ likewise represent the demand-curve of the $N$ customers who are supposed initially to be dealing with the other monopolist; with similar conventions as to the abscissa $OA'$, $OB'$, $OC'$ $(= OA, OB, OC$ respectively).

Now if each monopolist were dealing independently of the other with half of the customers he would fix the price at $OP$, since his net profit $Np(1 - p)$ is a maximum. When $p = \frac{1}{2}$ the

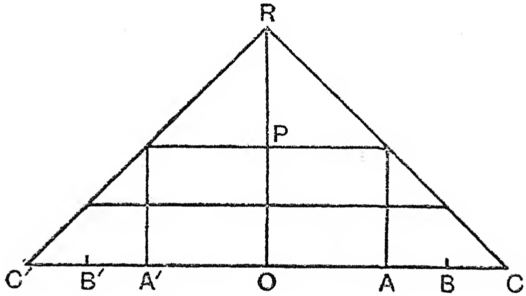


FIG. 1.

corresponding quantity would be $\frac{1}{2}c$. Let us start from this position. If the commodities were quite uncorrelated we would stop there. But as things are, it will be the interest of one of the monopolists to lower his price by a little, say $\delta p$, so as to attract his rival's customers. Throwing his whole stock on the market, he would realise a greater profit than before, namely, $\frac{3}{4}c(\frac{1}{2} - \delta p)$. He would not indeed be able with his limited supply to satisfy the entire demand, namely $c(\frac{1}{2} - \delta p)$, evoked by the lowered price. But he would have deprived his rival of a great part of his initial custom. However, the rival will now follow suit with a still lower price. So by successive steps, by variations of price which may be supposed to occur from day to day, the price may be lowered to $OQ$, $= \frac{1}{4}$, which is just sufficient to take off the whole supply of one monopolist offered to half the market, consisting of $N$ customers. At this point it might seem that equilibrium would have been reached. Certainly it is not the interest of either monopolist to lower the price still further.

But it is the interest of each to raise it. At the price $\frac{1}{4}$ set by one of the monopolists he is able to serve only $N$ customers (say the first $N$ on a queue) out of the total number $2N$. The remaining $N$ will be glad to be served at any price (short of unity, $= OR$). The other monopolist may therefore serve this remainder at the price most advantageous to himself, namely $\frac{1}{2}$. He need not fear the competition of his rival, since that rival has already done his worst by putting his whole supply on the market. The best that the rival can now do in his own interest is to follow the example set him and raise his price to $\frac{1}{2}$. And so we return to the position from which we started and are ready to begin a new

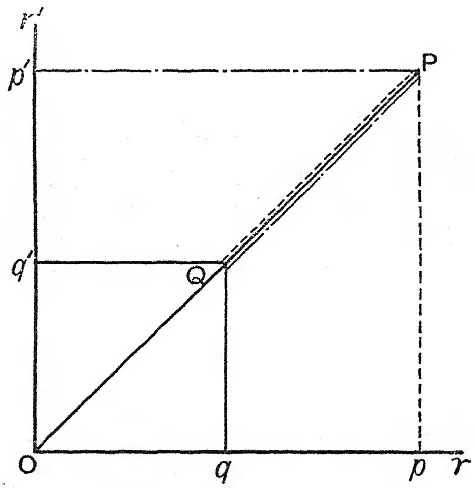


FIG. 2.

cycle. This need not have exactly the same path as that which we have described. For at every stage in the fall of price, and before it has reached its limiting value $\frac{1}{4}$, it is competent to each monopolist to deliberate whether it will pay him better to lower the price against his rival as already described, or rather to raise it to a higher, perhaps the initial, level for that remainder of customers of which he cannot be deprived by his rival (owing to the latter's limitation of supply). Long before the lowest point has been reached, that alternative will have become more advantageous than the course first described.

The matter may be put in a clearer light by taking $\xi$ and $\eta$ as co-ordinates representing the prices of the articles which are in the limiting case now considered identical, but in general only rival. The *dotted* lines in Fig. 2 represent the locus of maximum

profit for the monopolist owning the commodity $x$, of which the price is $\xi$—the *watershed*, so to speak, of the utility surface for that monopolist (or more exactly the locus of that price of $x$ which for any assigned price of $\eta$ affords maximum profit to the owner of $x$).* The corresponding locus for the second monopolist is represented by the *broken* lines. Corresponding to the data above defined, put $Op = Op' = \frac{1}{2}$; $Oq = Oq' = \frac{1}{4}$.

If we start from a price $Or$, above $Op$, the same for both, it will be the interest of one monopolist to lower his price to $Op$. The other monopolist from a similar motive, and faced with the loss of custom, follows suit. So we come to the point $P$, the position of equilibrium if the two markets were separate, or if the two monopolists were in combination. Now it is the interest of the seller of $x$ to lower his price by a little and so (the price of $y$ remaining the same) to move to the point where the dotted line parallel to $PQ$ intersects the broken line $Pp'$. The seller of $y$ then lowers his price $\eta$ to a point on the broken line which hugs the diagonal on the right. And so the system may dance down to the point which corresponds to the price $Oq$ ($= Oq'$), below which there is no tendency for the price to be lowered. But before this limit has been reached, the first price may have jumped back to the border-line $Pp$. The second will then presumably jump on to the line $Pp'$; and so *perpetual motion* is set up.

It will readily be understood that the extent of indeterminateness diminishes with the diminution of the degree of correlation between the articles. The illustration above given may be adapted to exhibit this incident.†

In the limiting case of no correlation between the commodities the locus of maximum advantage for each monopolist becomes a line parallel to one of the axes. For instance, if $Oa$ in Fig. 3 is the value of $\xi$ which affords maximum profit to the owner of $x$ when $\eta$, the price of $y$, is zero, $Oa$ continues to do so when $\eta$ varies; $aQ$ is the locus of maximum advantage for the owner of $x$.

B. The case of complementary demand may be illustrated by

* In general the maximum value of $\xi$ would depend not only on the assigned value of $\eta$, but also on the value of $y$.

† The figure is adapted only to cases which are adjacent to the one discussed in the text: suppose two sources of just distinguishable mineral waters which are supplied by two competing monopolists without cost of production. Some notion of the complications which arise when these simplifying suppositions are removed may be obtained from the example considered in the ECONOMIC JOURNAL, 1922; where it should be remarked that the quantities supplied, not as here the prices, are taken as the variables.

supposing $2N$ homogeneous customers whose laws of demand are for the first article :—

$$x = 2N(1 - \xi - \alpha\eta),$$

and for the second article :—

$$y = 2N(1 - \beta\xi - \eta).$$

To begin with, $\alpha$ and $\beta$ may be supposed very small and equal. Then the loci of maximum profit are for the respective monopolists :—

$$1 - 2\xi - \alpha\eta = 0;$$
$$1 - \alpha\xi - 2\eta = 0.$$

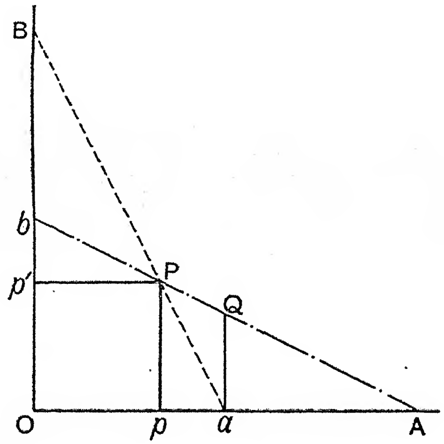


FIG. 3.

They may be imagined (they are not shown) as passing respectively through $A$ and $a$ in Fig. 3; the first almost vertical, the second almost horizontal.

The limiting case in which the two articles are perfectly complementary may be represented by putting $\alpha$ equal to 1. This is the case considered by Cournot when he supposes that each commodity has only one use; namely, to enter in a fixed proportion into the composition of a certain article for which there is a demand.[1] There are then given two monopolists who

[1] *Recherches*, ch. ix. It may excite surprise that when Cournot treats of two monopolists dealing in two perfectly rival articles, he supposes the steps towards equilibrium to be made by varying one *quantity* while the other remains constant (ch. vii.); whereas when he treats of two monopolists dealing in two articles perfectly complementary, he supposes that the steps are made by varying one of the *prices* while the other remains constant. An explanation may be found in the term "perfectly." If the articles are perfectly rival (that is, identical) there cannot well be supposed two prices; and if the articles are perfectly complementary (as in the case to which this note refers) there cannot be supposed two (independent variations of the) quantities.

offer in a market of competitive purchasers two complementary articles which enter in definite proportions $m_1 : m_2$ into the composition of an article for which the demand is $F(p)$, or $F(m_1p_1 + m_2p_2)$, where $p_1$ and $p_2$ are the prices of the complementary articles.* Abstracting cost of production we have for $U$ the gain of one monopolist, and $V$ the gain of the other :—

$$U = p_1F(m_1p_1 + m_2p_2);$$
$$V = p_2F(m_1p_1 + m_2p_2).$$

According to Cournot the prices are determined by the simultaneous equations

$$(1)\ \left(\frac{dU}{dp_1}\right) = 0 \quad (2)\ \left(\frac{dV}{dp_2}\right) = 0$$

(the price $p_1$ only being varied in $U$, and $p_2$ only in $V$). To which it may be objected that these equations cannot hold good simultaneously. Suppose, for instance, that the first holds good, then the second will not. For why should the second monopolist stop at the point at which the partial differential coefficient $\left(\frac{dV}{dp_2}\right) = 0$? He will go on varying the price $p_2$ up to the point at which the *complete* differential coefficient of $V$ is zero. That is

$$\left(\frac{dV}{dp_2}\right) + \left(\frac{dV}{dp_1}\right)\frac{dp_1}{dp_2} = 0;$$

where $\frac{dp_1}{dp_2}$ is derived from equation (1). This equation combined with (1) will determine $p_1$ and $p_2$.

To adapt Cournot's illustration to our scheme of rectilinear demand-curves, we may, without loss of generality put

$$m_1 = m_2 = 1;$$

and write for the demand of the first commodity

$$x = 2N(1 - \xi - \eta);$$

and likewise for the second commodity

$$\eta = 2N(1 - \xi - \eta);$$

where $\xi$ and $\eta$ are the respective prices. Then the position of maximum profit to the seller of $x$ for any assigned value of $\eta$ is given by the equation

$$1 - 2\xi - \eta = 0,$$

* The reader may like to have a reference to a real case of complementary articles (links in a chain of canals) owned by different (monopolist) companies; of which one fixes a high rate which "obliges the other companies to reduce their rates." Report on Railways and Canals Amalgamation, 1846, p. 200 (Vol. XIII.), Part IV. The concrete case is, however, not so simple as the one above imagined.

represented by the dotted line $Ba$ in the figure; and the corresponding locus for the seller of $y$ is

$$1 - \xi - 2\eta = 0.$$

If we suppose that both these equations exist simultaneously we ought to have $\xi = \eta = Op = Op'$; with $P$ as the position of equilibrium. If we suppose that one only of the equations holds good, the second, for example, but not the other, then, the first monopolist varying his price consistently with the satisfaction of the second equation, the position of equilibrium will be such that $\xi(1 - \xi - \eta)$ should be a *maximum, subject to the condition* that $1 - \xi - 2y = 0$; which gives that point on the line $Ab$ for which $\xi = \frac{1}{2}$; that is, the point $Q$, if $OA = 1$. But it is the better opinion, I think, that neither of these suppositions is tenable. For clearly in the case of a single monopolist, when it is laid down as a fundamental principle that $pF(p)$ (less cost of production) should be a maximum, it is not supposed that the demand $F(p)$ should be subject to the condition that the prices of all the other articles should remain constant when there are other articles whose prices vary with $p$. We have already had an example in the international trade above described.* Here is a further somewhat fanciful illustration.

Suppose Nansen and Johansen are dragging their sledge over the Arctic plains (all their dogs having died). In the pursuit of different scientific aims one of them, Nansen, tries to get up on the ice as far as possible above the level of the sea, while the other strives to reach the position at which the depth of the sea measured from the sea-level is a maximum. With these different objects Nansen and Johansen do not act in concert; so much only of their old partnership remains that they do not act against each other, Nansen moves only in a line of latitude (in either direction), Johansen only in a line perpendicular thereto, a line of longitude, parallel to axis $OB$.

Under these conditions it is very possible that the two surfaces —of the ice and of the bottom of the sea—are crumpled in such wise that the sledge will never come to a point such that neither of the parties will want to get away from it. Such was the case above described with reference to rival commodities.

There is also possible another case. Suppose the principal ridge of ice on which Nansen wants to get as high as possible runs in the direction $Ba$ (Fig. 3), and that the principal valley in the bottom of the sea above which Johansen wants to get as

* See above, p. 114.

high as possible runs in the direction $bA$. The intersection of these two lines at the point $P$ might seem to be a position of equilibrium. From this point it is not the interest of Nansen to move either to the right or left; nor of Johansen to move either up or down. Nevertheless if Nansen were to move from this position—whether by accident in the polar darkness, or designedly foreseeing a future move of Johansen—say to the right to a neighbouring point $P_1$, not shown in the figure; then Johansen would tend to move downwards on the vertical line through $P_1$ to a point $Q_2$, where the depth of the valley measured from the sea-level is greatest. At this point it is probable that the height of the ice is greater than it was at the initial point $P$, since the "hog's-back" formed by the ice becomes higher as one moves towards $OA$ along its crest, and accordingly as one moves near the crest, in that downward direction. Nansen will then be in a position to repeat his step to the right, whether induced by a knowledge of Johansen's motives, or simply by the fact that his first step to the right resulted in advantage to himself.* And so there may be reached a point on the line $bA$ considerably below and to the right of the initial point $P$, the point at which it will no longer prove to the advantage of Nansen to take a step to the right. At this point, which proves to be that at which $\xi = \frac{1}{2}$, the point $Q$ in Fig. 3, it may be thought that equilibrium will finally have been reached.

But it will not be a stable equilibrium, except on the extreme supposition that Nansen is perfectly intelligent and foreseeing, while Johansen, as the saying is, "cannot see beyond his nose." Otherwise let us suppose *first* that both proceed by tentative steps in the dark. At the point $Q$, or perhaps before getting so far from $P$, the immediate interest of Nansen may prompt him

* Let $Q_1$ be the point on the line $Ba$ at which Johansen moving downwards from the point $P_1$ stops. The step $PP_1$ being short the position $Q_1$ must be more advantageous for Nansen than $P$ from which he started. For $Q_1$ is *within* the curve of constant advantage, the indifference-curve, may we say, pertaining to Nansen defined by the equation $U =$ constant where $U = \xi 2N(1 - \xi - \eta)$. Whence

$$\frac{d\eta}{d\xi} = -\frac{du}{d\xi}\Big/\frac{du}{d\eta} = -\frac{1-2\xi-\eta}{-\xi}.$$

Thus the tangent to Nansen's indifference-curve (which is concave towards the axis $Oa$) is horizontal at $P$ (since at that point $\xi = \eta = \frac{1}{3}$); and at $P_1$ it slopes slightly downwards to the right, but not nearly so much as the line $bA$. Accordingly, if Nansen makes a second short step to the right from $Q_1$, say to $P_2$, and thence Johansen moves down to $Q_2$ on $bA$, the position $Q_2$ will be more advantageous for Nansen than $Q_1$. And it can be shown that this downward movement may continue on to the point $Q$ where the tangent to the indifference-curve becomes coincident with the line $bA$.

to move to the left to a point on the line *Ba*. From this point Johansen will move upwards to a point on the line *bA*. And so on, until they regain perhaps the initial position *P*; ready to start on a second excursion, this time perhaps in an upward direction. *Secondly*, let us suppose that both are perfectly intelligent and aware of each other's motives. Then from the point *Q*, for instance, it is quite possible that Johansen may move *upwards*; not that it is his immediate interest to move in this direction, but in the hope of inducing Nansen to move to the left to a position (on the line *bA*) more advantageous to Johansen than *Q*. Nansen, however, may not lend himself to this plan. And so the two may continue to make moves against each other; or if they stop, it will be only for a time, and not in a determinate position.*

To drop metaphor, it is certain in the case of rival articles offered by monopolists not in combination, and at least very probable in the case of complementary articles, that economic equilibrium is indeterminate.

It is unnecessary to point out how prevalent in the actual world are the relations of "rival" and "complementary." Let the reader consider the passages referred to in Professor Marshall's *Principles* under headings "Joint Demand," and "Substitutes." It will be sufficient here to mention two cases which, though they do not possess the essential characteristics of rival or complementary goods as above defined, yet have the property of rendering monopoly price unstable. The *summa genera* of necessary articles, food, clothes and so forth, may be regarded as complementary in a certain sense in so far as an increase in the price of one class tends to diminish that of the other class. For instance, it is said that during a dearth in one of our northern cities the price of old clothes diminished. Articles of consumption may also be rivals in a sense, though not capable of acting as substitutes for each other, if an increase in the price of one causes less money to be spent on it, and the money thus set free goes to increase the price of other articles.†

SECTION III.—Since then there is no theory of economic equilibrium in the case with which we have to do with different monopolists, we may confine ourselves to the case in which there is only one monopolist in the field. An important variety of this case occurs when there are two or three different markets

* For further illustrations of the indeterminateness which is characteristic duopoly see ECONOMIC JOURNAL, September 1922.

† *Cp.* below, p. 137.

furnished by one and the same monopolist with two or more articles of which the production is *joint* in this sense, that the increase of one renders the increase of the other ($a$) more, or ($b$) less, costly. In symbols let $x$ and $y$ be the respective quantities produced and $\varphi(x,y)$ the expenses, or, more generally, the pecuniary measure of the real cost of the productions of $x$ and $y$ together; we have then case ($a$) if $\frac{d^2\varphi}{dx,dy}$ is positive; if it is negative, case ($b$). These relations may be designated by the terms ($a$) rival production, ($b$) complementary production.

It should be observed that "complementary production" as here defined is not identical with joint production as used by some distinguished writers. If the expense incident to the production of two articles in the quantities $x$ and $y$ is $C + ax + by$, where $C$, $a$ and $b$ are constants, these articles would commonly be described as produced jointly, but they are not "complementary in our sense.*

($a$) First, the production being rival, let the cost of producing $x$ together with $y$ be $\varphi(x,y)$ where $\frac{d^2\varphi}{dx^2}$, $\frac{d^2\varphi}{dy^2}$ (the law of increasing cost being assumed) and $\frac{d^2\varphi}{dx\,dy}$ are each positive. Let $f_1(x)$ be the price at which the quantity $x$ is demanded in one market and $f_2(y)$ the price at which the quantity $y$ is demanded in another market; then if $V$ is the net advantage of the monopolist,

$$V = xf_1(x) + yf_2(y) - \varphi(x,y).$$

Now suppose a small tax of $u$ per unit is imposed on the first commodity. If $dx$ and $dy$ are the consequent variations in the quantities furnished we have,

since
$$\left(\frac{dV}{dx}\right) = 0, \text{ and } \left(\frac{dV}{dy}\right) = 0,$$
$$dx\frac{d^2V}{dx^2} + dy\frac{d^2V}{dx\,dy} = u,$$
$$dx\frac{d^2V}{dx\,dy} + dy\frac{d^2V}{dy^2} = 0;$$

whence $dx = u\frac{d^2V}{dy^2} \div \Delta$; $dy = -u\frac{d^2V}{dx\,dy} \div \Delta$; where $\Delta$ is the determinant $\frac{d^2V}{dx^2}\cdot\frac{d^2V}{dy^2} - \left(\frac{d^2V}{dx\,dy}\right)^2$, a quantity which must be positive in order that $V$ should be a maximum. For the same

* Compare the definitions adopted by Pigou; as to which see passage referred to in Index, s.v. *Joint Production.*

reason $\frac{d^2V}{dx^2}$ must be negative. Also $\frac{d^2V}{dx,dy} = -\frac{d^2\varphi}{dx,dy}$ must be negative. Accordingly $dx$ is negative, $dy$ is positive; the purchasers of the taxed article are damnified, while the purchasers of the tax-free article are benefited. The proposition may be extended to any number of articles.

We might have reached the same conclusion if we had treated the *prices*, say $\xi$ and $\eta$, as the independent variables; in which case it would be proper to substitute for $x$, $F_1(\xi)$ and for $y$, $F_2(\eta)$, likewise for $f_1(x)$ and $f_2(y)$ respectively $\xi$ and $\eta$.

Analytical geometry may be usefully employed with either set of variables. Thus let $V$ be represented by the height of a surface depending on the independent variables $\xi$ and $\eta$. The position of maximum height is given by the simultaneous equations

$$(1)\ \left(\frac{dV}{d\xi}\right) = 0; \quad (2)\ \left(\frac{dV}{d\eta}\right) = 0$$

These equations are adequately represented, with respect to values of the variables, in the neighbourhood of the maximum by the curves $AA'$ and $BB'$ in Fig. 4. For both the curves in the neighbourhood of $P$ will be inclined negatively to the axis of $\xi$; that is, the tangent $\frac{d\eta}{d\xi}$ will be for both negative. Further, that tangent *in absolute quantity* will be greater for $AA'$ than for $BB'$. For with respect to $AA'$

$$\frac{d\eta}{d\xi} = -\left(\frac{d^2V}{d\xi^2}\right) \div \left(\frac{d^2V}{d\xi\, d\eta}\right)\left(\text{since } \left(\frac{dV}{d\xi}\right) = 0\right)$$

The numerator of this fraction is positive since $V$ is a maximum (at the point $P$). Also the denominator is negative, as may be seen by substituting $F_1(\xi)$ and $F_2(\eta)$ for $x$ and $y$ in $\varphi(x,y)$. By parity of reasoning the tangent for $BB'$

$$= -\left(\frac{d^2V}{d\xi\, d\eta}\right) \div \left(\frac{d^2V}{d\eta^2}\right) < 0.$$

These values of $\left(\frac{d\eta}{d\xi}\right)_1$ and $\left(\frac{d\eta}{d\xi}\right)_2$, as they may respectively be called, are now to be combined with the *third* condition required in order that $V$ may be a maximum, viz.

$\frac{d^2V}{d\xi^2} \cdot \frac{d^2V}{d\eta^2} > \left(\frac{d^2V}{d\xi\, d\eta}\right)^2$. Whence $-\left(\frac{d\eta}{d\xi}\right)_1 \Big/ -\left(\frac{d\eta}{d\xi}\right)_2 > 1$; and $\left[\left(\frac{d\eta}{d\xi}\right)\right]_1$ in absolute quantity, $> \left[\left(\frac{d\eta}{d\xi}\right)\right]_2$. Thus $\left(\frac{d\eta}{d\xi}\right)_1$ and $\left(\frac{d\eta}{d\xi}\right)_2$

being both negative, the curves ought to be (in the neighbourhood of the *maximum*) inclined to the axes and to each other as represented in Fig. 4.

Now let a (small) tax of $u$ *ad valorem** be imposed on the $x$ commodity. The curve $AA'$ will be displaced to the right as in the figure; while the curve $BB'$ remains unchanged. Thus while $\xi$ is increased $\eta$ is diminished; a conclusion identical with that reached above, since the prices and quantities vary inversely.*

If we had treated $x$ and $y$ as the independent variables, the loci $\left(\frac{dV}{dx}\right) = 0$, and $\left(\frac{dV}{dy}\right) = 0$ would still have been related like

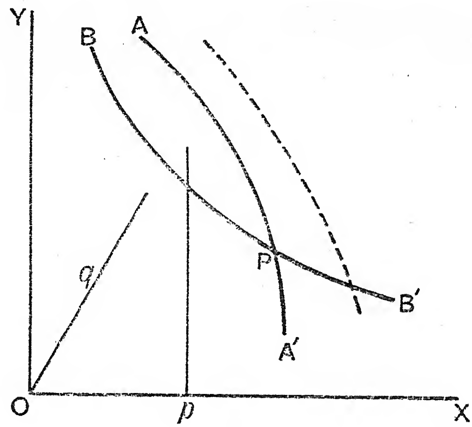


FIG. 4.

the curves $AA'$ and $BB'$ in Fig. 4; while the displaced curve would lie on the *left* of $AA'$.

Conversely it may be shown that a bounty to one of the commodities will prejudice the consumers of the other.

The effects of other kinds of governmental control may be studied by a similar procedure. Thus let there be prescribed a *maximum* price or a *fixed* price for one of the articles, a price less than what would have been reached if monopoly were allowed free play. If $\xi$ in Fig. 4 is limited to $Op$, less than $OP$, the position of equilibrium will be the highest point on the curve formed by the intersection of the surface ($z = V$) with a plane through $p$ perpendicular to the axis $OX$. The ordinate of the curve $BB'$ formed by its intersection with a perpendicular through $p$ (in the plane of $\xi\eta$)

* The conclusion is readily extended to a specific tax by substituting in the above for $\Delta\xi$, considered as a small percentage of $\xi$, the increment $\Delta x \frac{d\xi}{dx}$. Like reasoning applies to other small taxes.

to the axis $OX$ (the price $\eta$) will evidently be greater than the ordinate intersecting at $P$. Of course if $Op$ is considerably less than $OP$ it might happen that the vertical plane through $p$ does not meet the surface above the plane of $\xi\eta$. The value of $V$ then becomes negative or impossible; the business cannot go on.

Very similar is the effect of the condition that one price should not exceed the other by more than a certain proportion. This condition is exemplified by the American short-haul clause; which enacts that if $D_1$ the distance of one station (from the terminus) is less than $D_2$, the distance of another station, and $\xi\eta$ are the respective fares per mile, then $D_1\xi$ shall not exceed $D_2\eta$.

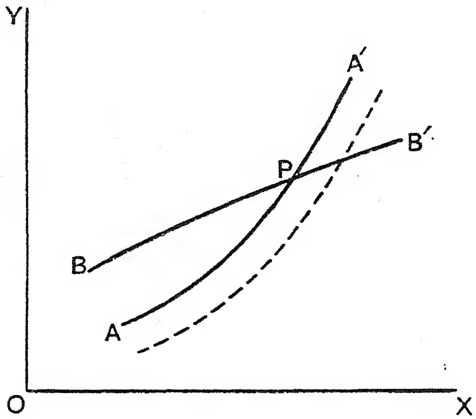


FIG. 5.

In other words, $\frac{\xi}{\eta}$ is not greater than $\frac{D_2}{D_1}$. This limit may be expressed by a line through the origin such as $Oq$ in Fig. 4.

A line not passing through the origin may represent the condition that the *difference* between the two prices should not exceed a certain maximum.

($b$) Corresponding propositions may be demonstrated for articles of which the production is complementary. We have simply to change the sign of $\frac{d^2\varphi}{dx\,dy}$; and accordingly the inclination of the price-curves, which will now be inclined and related as $AA'$ and $BB'$ in Fig. 5. The displacement caused by a tax on the $x$ commodity is represented by a dotted curve on the right of $AA'$ (on the understanding that the axes represent prices). Whence it appears that a tax on one of the complementary articles will cause the price of *both* to rise.

So far, supposing that the consumers of $x$ and $y$ constitute two distinct classes.[1]

Let us now suppose that the separation of classes no longer exists; and first let us suppose that $x$ and $y$ are quantities of rival commodities offered on a single market. For instance, $x$ and $y$ may denote respectively travelling on a railway by first or second class. (For simplicity we may suppose that there are only two classes, as commonly now in England; though they are commonly called first and *third*). This case is analogous to the preceding in so far as a tax on one commodity diminishes the quantity thereof which will be put on the market. But it does not now follow that the consumers of the substitute will be benefited. The consumers *in globo*, for instance, travellers on the railway as a body, may be prejudiced by a tax on one of the commodities, say travelling by first class; but it is also possible that they should be *benefited* thereby. The first proposition is self-evident; the second is a paradox which can only be demonstrated with the aid of mathematics.

Let $f_1(x,y)$ be the price of the first commodity when $x$ and $y$ are the quantities of the respective commodities that are taken by the market. Then $\dfrac{df_1}{dx}$ is, of course, negative. Also $\dfrac{df_1}{dy}$ is negative, since the increased consumption of $x$ diminishes the demand for the substitute $y$. Let $f_2(xy)$ likewise represent the price of $y$. Then for the total net profit of the monopolist we have

$$V = xf_1(x,y) + yf_2(x,y) - \varphi(x,y).$$

For $\Delta x$ and $\Delta y$, that is, the increments of the commodities due to a small tax $\tau$ on $x$, we have as before

$$\Delta x \frac{d^2 V}{dx^2} + \Delta y \frac{d^2 V}{dx\,dy} = \tau$$
$$\Delta x \frac{d^2 V}{dx\,dy} + \Delta y \frac{d^2 V}{dy^2} = 0.$$

From which it appears as before that $\Delta x \left(= \tau \dfrac{d^2 V}{dy^2}\Big/ D\right.$ where $D$ is positive) is negative. But $\Delta y$ may be either positive or

[1] An important variety of this case occurs when a monopolist fixes different prices for the same article as consumed by different classes; for instance, a ticket for a theatre may bear a different price according as it admits a soldier or a civilian, a man or a woman. Many interesting examples of this type are adduced by Neumann in Schönberg's *Handbuch* (see an example given by *Dupuit*, below II. 404).

negative. All that can be said with certainty about its sign is that it will be contrary to the sign of $\frac{d^2V}{dx\,dy}$. But nothing is known about this second differential, except that it must satisfy the condition for $V$ being a maximum, viz.—

$$\left(\frac{d^2V}{dx^2}\right)\left(\frac{d^2V}{dy^2}\right) - \left(\frac{d^2V}{dx\,dy}\right)^2 > 0;$$

a condition which does not depend on the *sign* of the quantity which is squared.* This condition is compatible with the supposition that not only should $\Delta y$ be positive, but also $x\Delta\xi + y\Delta\eta$,[1] the approximate expression for the decrement of Consumers' Surplus, should be negative; that is, the consumers as a whole should be advantaged by the tax.

As, even with respect to mathematics, "seeing is believing," I subjoin a numerical example of the special case.** Let $x$ and $y$ be the quantities of two commodities which are rivals in consumption (partial substitutes for each other). Let the law of demand of these commodities be as follows, $p_1$ and $p_2$ being the respective prices

$$p_1 = 1\cdot605\dot{3} - \cdot2x - \tfrac{2}{3}\,(x - \cdot96)^{\frac{3}{2}} - \tfrac{1}{2}\,y$$
$$p_2 = 3\cdot9\dot{1}\dot{8} - 2(y - \cdot6975)^{\frac{1}{2}} - \tfrac{1}{2}\,x$$

for values of $x$ and $y$ in the neighbourhood of the vaules $x = 1$ and $y = 1$. This is a rational supposition, since there exists a function $U$ such that $\left(\frac{dU}{dx}\right) = p_1$ $\left(\frac{dU}{dy}\right) = p_2$; and $U$ is suited

* Nothing can be learnt about the sign in question from the laws of utility, since they tell us only that, if $U$ is the Total Utility or Consumers' Surplus (*cp.* note to p. 117), $\frac{dU}{dx} > 0$, $\frac{dU}{dy} > 0$, $\frac{d^2U}{dx^2} < 0$, $\frac{d^2U}{dy^2} < 0$, $\frac{d^2U}{dx^2}\,\frac{d^2U}{dy^2} - \frac{d^2U}{dx\,dy} > 0$; whereas $\frac{d^2V}{dx\,dy}$ involves *third* differentials of $U$, about which nothing is given.

[1] The total net utility accruing to the consumers or the Consumers' Surplus obtained from the purchase of the quantities $x$ and $y$ at the prices $\xi$ and $\eta$ respectively, may be written $U - x\xi - y\eta$; where $U$ is identical with $F(x_1y)$, as defined on p. 117 above. The total net utility when $x + \Delta x$ is substituted for $x$ and for $y$ $y + \Delta y$, becomes (approximately)

$$U + \Delta x\left(\frac{dU}{dx}\right) + \Delta y\left(\frac{dU}{dy}\right) - (x + \Delta x)(\xi + \Delta\xi) - (y + \Delta y)(\eta + \Delta\eta)$$
$$= U - x\xi - y\eta + \Delta x\left(\frac{dU}{dx} - \xi\right) + \Delta y\left(\frac{\Delta U}{dy} - \eta\right) - (x\Delta\xi + y\Delta\eta).$$

Whence the increment to the total net utility due to the increments of $x$ and $y$ is $-(x\Delta\xi + y\Delta\eta)$ (since $\left(\frac{dU}{dx}\right) = \xi$ and $\left(\frac{dU}{dy}\right) = \eta$).

** The numerical data here used are not exactly the same as those given in the example as originally set forth in the *Giornale*. The figures are now taken from a simplified version of that example (presented below, **F**, p. 148). Further fortification of the theory is offered at II. 93, **S**; and a fresh example at II. 400, $\zeta$.

to represent the total utility (above a certain minimum) derived from the possession of the quantities of $x$ and $y$ distributed as described above (p. 117). For as $\left(\frac{dp_1}{dx}\right) = \left(\frac{d^2U}{dx^2}\right)$ is negative in the neighbourhood of the values $x = 1$, $y = 1$ (for which values $\left(\frac{dp_1}{dx}\right) = -\cdot 4$), there is, as there ought to be, a limit to the quantity of $x$ which the consumers will take at that price, supposing the price of $y$ to be fixed. There is a corresponding limit to the increase of $y$ since $\left(\frac{dp_2}{dy}\right)$ is negative ($=-1\cdot\dot{8}\dot{1}$). Further, supposing both quantities to vary simultaneously, there is, as there ought to be, a limit to the amount of *sandwiches* of the form $lx + my$ which the consumers at any assigned (pair of) prices will demand; since the remaining condition for $U$ being a maximum holds good, for $x = 1$, $y = 1$ (and in the neighbourhood), viz.—

$$\left(\frac{d^2U}{dx^2}\right), \left(\frac{d^2U}{dy^2}\right) - \left(\frac{d^2U}{dx\,dy}\right)^2 > 0.$$

Such being the laws of demand, we have for the monopoly profit $V = xp_1 + yp_2$, *i. e.* supposing at first that there are no expenses of production; which is a maximum when $x = 1$, $y = 1$, since then

$$\left(\frac{dV}{dx}\right) = 1\cdot605\dot{3} - \cdot 4x - \tfrac{2}{3}(x - \cdot 96)^{\frac{3}{2}} - x(x - \cdot 96)^{\frac{1}{2}} - y = 0,$$

$$\left(\frac{dV}{dy}\right) = 3\cdot9\dot{1}\dot{8} - 2(y - \cdot 6975)^{\frac{1}{2}} - y(y - \cdot 6975)^{-\frac{1}{2}} - x = 0;$$

while the second differential coefficients of $V$ fulfil the remaining condition for a maximum; for

$$\left(\frac{d^2V}{dx^2}\right) = -\ 3\cdot 3$$

$$\left(\frac{d^2V}{dy^2}\right) = -\ \cdot 6311$$

$$\left(\frac{d^2V}{dx\,dy}\right) = -\ 1$$

$$(-\ 3\cdot 3) \times (-\ \cdot 6311) - 1^2 = 1\cdot 0826 > 0.$$

If now a small tax of $\tau$ per unit is imposed on the first commodity we have for the increments of quantity (above, p. 131)

$$-\ 3\cdot 3\ \Delta x - \Delta y = \tau$$

$$-\ \Delta x - \cdot 6311\ \Delta y = 0.$$

Whence

$$\Delta x = -\tau \cdot 6311 \div 1\cdot 0826 = -\cdot 5829\tau;\ \Delta y = 1 \div 1\cdot 0826 = +\cdot 9237\tau.$$

Accordingly the *decrement* of Consumers' Surplus

$$\begin{aligned} &= x\Delta p_1 + y\Delta p_2 \text{ (approximately)} \\ &= \Delta x\left(x\frac{dp_1}{dx} + y\frac{dp_2}{dx}\right) + \Delta y\left(x\frac{dp_1}{dy} + y\frac{dp_2}{dy}\right) \\ &= -\cdot 583\,(-\cdot 4 - \cdot 5)\tau + \cdot 9237\,(-\cdot 5 - 1\cdot\dot{8}\dot{1})\tau \\ &= -1\cdot 626\tau. \end{aligned}$$

Since then the *decrement* of the *Consumers' Surplus* is negative, there is a positive increment of advantage to the consumers in consequence of the tax. Or is it easier to say that as *both* the prices are reduced, the purchasers must be gainers? *

The conclusion becomes *a fortiori* when there are expenses of production; for then we have at our disposal more functions with which to manipulate a favourable example.**

Thus a tax on first-class tickets may have the effect of lowering the fares for both first and third class, and so benefiting passengers in general. The number of travellers by first class will, however, be diminished notwithstanding the attractions of a lower fare; the counter attractions of the lowered second-class fares predominating.

The paradox which has been exhibited is presented by many other kinds of taxation, or more generally governmental regulation relative to commodities that are correlated in consumption. The correlated commodities need not be *rivals*, as in the preceding example; they may be *complementary*, such as the carriage of a passenger's luggage and the carriage of the passenger himself. Likewise a bounty on one of the correlated commodities may prove *injurious* to the consumers.*** Again, the limitation of the monopoly profit to a fixed percentage of the cost (including interest on capital) is not necessarily advantageous to the consumer. For the problem is then to maximise $V$ subject to the condition that**** $V \ngtr i\varphi(x, y)$, where $i$ is a given fraction. Then beginning with the case in which the fraction $i$ is such that the limitation is only just beginning to be operative, we shall find as before that the variations in the Consumers' Surplus consequent upon the limitation depend upon the sign of the magnitude $\left(\frac{d^2V}{dx,dy}\right)$ (or the corresponding second differential coefficient with respect

* $\Delta p_1 = -\cdot 229\tau$, $\Delta p_2 = -1\cdot 4\tau$.

** The example is modified so as to illustrate this point in the article dated 1899, which is republished below, F, p. 149; where the $\xi$ and $\eta$ are used in the same sense as $x$ and $y$ in the present context.

*** Not stated explicitly in the original.

**** The symbol $\ngtr$ (not greater than) expresses the limitation better than the symbol $=$ used in the original.

to $\xi$ and $\eta$); which in general is not given. The principle applies very generally to the taxation of correlated consumable articles in a regime of monopoly.*

These paradoxes may be somewhat diminished by the use of a principle which Economics is entitled to borrow from the kindred science of Probabilities, or the "Art of Conjecturing." This is the presumption that in certain cases a quantity of which we do not know the sign may be treated as zero. In the leading case before us, if, as before, $f_1(x_1y)$ is the price of the first commodity and $f_2(xy)$ that of the second (when the quantities $x$ and $y$ are taken by the market), $\varphi(x_1y)$ is the total cost of producing $x$ and $y$, and $V$ the net advantage of the monopolist, we have

$$V = xf_1(xy) + yf_2(xy) - \varphi(xy);$$

$$\frac{d^2V}{dx\,dy} = \left[\left(\frac{df_1}{dy}\right) + \frac{df_2}{dx}\right] + \left[x\frac{d^2f_1}{dx\,dy} + y\frac{d^2f_2}{dx\,dy}\right] - \left[\frac{d^2\varphi}{dx\,dy}\right].$$

Of the three parts or terms of this expression (distinguished by square brackets), we know that the first is positive or negative according as the demand is complementary or rival; and that the third (with its sign) is positive or negative according as the production is complementary or rival. But we do not know the sign of the second term; and are therefore perhaps justified in ignoring it; especially when the sum of the two other terms is considerable, as may well be if production and consumption are either both rival or both complementary.

So far in this section we have been supposing that the monopolist, true to his etymology, is only a *seller*. But the method which has been indicated may readily be extended to the case in which the monopolist is the *buyer* of two or more correlated commodities. Thus it is possible that he may have a rival or complementary demand for goods supplied by distinct groups of producers. Or he may purchase goods of which the production is rival or complementary. And at the same time he may have a rival or complementary demand for those goods.

We may form any number of combinations with the attributes of which the properties have been deduced; always excepting those cases in which two or more monopolists are in the field.

SECTION IV.—In conclusion I now propose to restate in plain

* The exposition in the original is interrupted by the statement of two well-known or obvious propositions: (*a*) A progressive (as well as a simply proportional) tax on the profits of the monopolist does not affect the consumers (even in the case of correlated consumption). (*b*) The effect of limiting the monopoly profit to a fixed amount is indeterminate; consumers may be either benefited or prejudiced by the limitation.

words the principal results of the preceding mathematical analysis. They consist, as might be expected, rather in general views than in particular rules.

1. One of the principal uses of mathematics to the economist is, in the words of Professor Marshall, "to make sure that he has enough and only enough premisses for his conclusions (*i. e.* that his equations are neither more nor less in number than his unknowns)." This criterion applied to monopoly shows that frequently—I think it may almost be said normally—there is not a sufficient number of conditions to render economic equilibrium determinate, in the general case of a system of bargains in which more than one monopolist takes part. This may be affirmed with peculiar confidence in the case where two or more monopolists who are in competition deal with a great number of customers who also are competing with each other; for example, two railways which ply between the same points. The instability is due not merely to the hope of one monopolist to ruin a rival by "cutting prices," a case that has often been described; but also to a more fundamental, though less obvious cause. The instability does not cease in cases where it is not possible for one monopolist to drive the other completely off the field. Such might be the case if workmen of two nationalities—say Anglo-Saxons and Chinese—united respectively in two combinations, had to deal with competitive entrepreneurs, or with foreign customers. The proposition clearly stated by Cournot,[1] and to all appearance generally admitted, that in such a system the action of economic forces would tend to a definite position of equilibrium, a determinate set of values,—this plausible proposition is proved to be unfounded. In the regime of competition, as Mill or someone has said, things are always seeking their level. It is not so in the regime of monopoly.

The character of perpetual instability may likewise be affirmed of conditions in which the two competing monopolists deal not in identical, but rival, articles; for example, in the cases just now instanced it may be supposed that the services of the two railways, or the work of the two nationalities, though not quite identical, are capable of acting as more or less perfect substitutes for each other.

This theory is less evident, the opinion of Cournot is more plausible, in cases where the competing monopolists are dealing

[1] "Il est bien évident que dans l'ordre des faits réels et lorsque l'on tient compte de toutes les conditions d'un système économique, il n'y a pas de denrée dont le prix ne soit complètement déterminé."

not in rival but "complementary" articles; for example, if the rolling-stock of a railway were possessed by one company and the railway-stations by another, or if the common labour necessary for the production of an article were monopolised by one combination, and the more highly skilled work without which the manual labour would be useless by another combination. Professor Marshall seems to contemplate this case when he supposes that a mill belongs to one monopolist and the water for driving it to another.[1]

Let us suppose that the two lettings are yearly; beginning at the middle of the year for the mill, and at the end of the year for the water-supply. If at midsummer the owner of the premises, when renewing his contract with the lessee, estimates what such a one can pay, on the basis of what he pays and will pay for the next six months for the use of the water—if the owner of the mill ignores the possible action of the owner of the water at the end of the year—then perhaps the reasoning of Cournot in a similar case will hold good. There will be a determinate equilibrium characterised by the curious property that the tenant will be worse off than if both had belonged to the same individual. That is, supposing that there are a number of mills at the disposal of the landlord, and a number of millers competing with each other.

But ought we to suppose that the proprietor, when renewing his contract, does not take into consideration possible future events? Will he not, theoretically, fix the rent at that figure which will be the most advantageous for him *in view of the rent which the owner of the water-supply may fix the next winter?* It is thus that a chess-player when making his move takes account of the move which his adversary will probably make. And, as in chess, when only the two kings and one of the inferior pieces remain on each side, may not the two monopolists go on making moves against each other to all eternity?

Those who adhere to Cournot's reasoning may be confronted with the supposition that one of the two monopolised articles, for instance, the water-power in the above example, passes into the hands of competitors. There will then be a regular market for water-power, offered by the competing owners to competing millers. Accordingly, given the rent of the mill, the payment for water-power will be determined by the usual equation between demand and supply (the total supply of water-power may be supposed a fixed quantity). According to the opinion

[1] *Principles of Economics*, third ed., Book V. ch. x.

here disputed, when once the charge for water-power has been settled by the market, the monopolist will treat this price as something sacred, and will only vary the rent of his premises *subject to the condition that the charge for water-power should not be disturbed.* But surely the general rule is that he will continue to vary the price of the monopolised article as long as that price multiplied by the quantity sold at that price—less the cost of production—continues to increase. It does not matter to him that the customers, in view of his changing that price, are obliged to modify their bargains with a third party. What difference can it make to the motives of the monopolist that the third party consists of a monopolist, not of individuals competing against each other? In both cases, indifferent to the interests of the third party, he will vary his price by successive steps in the direction which promises him an increase of profit. The only difference between the cases is that when the third party consists of competitors, a definite position of equilibrium will be reached (the tentatives of the single monopolist must come to a stop, or at least hover about a determinate point); whereas when the third party consists of a second monopolist, the conditions which bring about the equation of demand and supply in a competitive market are wanting. That is, excepting the arbitrary supposition that the second monopolist is such a fool as to act in the manner ascribed to him by Cournot's equation. But even if he were to do so, though there would exist a definite position of equilibrium, it would not be the one assigned by the theory here combated.*

This theoretical difference between the regime of monopoly and that of competition may have some bearing on practical issues, affecting as it does our views about trade unions and similar combinations. I have seen it proposed as an economic ideal that every branch of trade and industry should be formed into a separate union. The picture has some attractions. Nor is it at first sight morally repulsive; since, where all are monopolists, no one will be the victim of monopoly. But an attentive consideration will disclose an incident very prejudicial to industry —instability in the value of all those articles the demand for which is influenced by the prices of other articles; a class which is probably very extensive.

Among those who would suffer by the new regime there would be one class which particularly interests the readers of this Journal, namely the abstract economists, who would be

* It would correspond to the point $Q$, not to $P$, in Fig. 3.

deprived of their occupation, the investigation of the conditions which determine value. There would survive only the empirical school, flourishing in a chaos congenial to their mentality.

2. Professor Marshall exemplifies another use of mathematical reasoning when by means of his curves he demonstrates that it might be advisable to tax one kind of commodity and employ the proceeds in bountying another kind.[1] The abstract reasoning serves as a corrective to what has been called the "metaphysical incubus" of dogmatic *laisser faire*. In the case of monopoly indeed this incubus has not been serious: "it has never been supposed that the monopolist in seeking his own advantage is naturally guided in that course which is most conducive to the well-being of society regarded as a whole." Nevertheless, in so far as something similar to the old doctrine of economic harmony seems to be reappearing among the apologists for railway administration, a certain interest may attach to propositions unexpectedly favourable to the intervention of Government in businesses subject to monopoly. Such is the proposition above proved, that when the supply of two or more correlated commodities—such as the carriage of passengers by rail first class or third class—is in the hands of a single monopolist, a tax on one of the articles—*e. g.* a percentage of first-class fares—may prove advantageous to the consumers as a whole. Thus in the instance given the advantage would accrue not only to those who before the tax travelled third class, and continue to do so afterwards, but the travelling public in general, including first-class passengers. The fares for *all* the classes might be reduced.

3. To obtain rules directly applicable to practice there would be required a knowledge of concrete details beyond what the present writer can command. Still, some suggestions bearing on the control of monopolies by governmental interposition may be derived from the preceding analysis.

A first step in this direction was made by Cournot when he proved that a tax of an ordinary kind on a monopolised product has the effect of increasing the price. This is contrary to the judgment of some distinguished writers who hold that, the monopolist having already done his worst against the customer, the burden of the latter cannot be increased by a tax. There is, however, a limiting case in which the popular opinion is correct; namely, where a monopolist buyer deals with sellers of an article which is absolutely limited in quantity (land, for instance), or can only be increased with great difficulty. A building syndicate

[1] *Principles*, Book V. ch. xii. pp. 555–7. *Ibid.* ch. xiii. (third ed.)

buying up land from uncombined owners may afford an example.

Cases specially favourable for the application of mathematical analysis occur where we have to deal with *correlated* (connected) supply and demand. Suppose, first, the supply only connected, as when a railway company, the greater part of whose expenditure (interest on capital, cost of repairs, etc.) cannot be attributed exclusively to one branch of the business, serves two classes of customers whose interests are quite separate, say traders requiring their goods to be carried and passengers other than commercial travellers. Here we must distinguish two classes: (*a*) *complementary* products, in the case of which the production of one article becomes less difficult and expensive by the increased production of the other article ("joint" products as defined by Mill are included in this class); (*b*) *rival* products, in the case of which the production of one article becomes more costly according as the production of the other is increased. The first case usually occurs where the law of increasing returns rules; for instance, if the general expenses of a railway do not increase in proportion to the traffic, the increase of one kind of traffic tends to make the increase of the other kind more remunerative (see above a more exact definition). Contrariwise, when the land or the capital at the disposal of the company is fully occupied, it is possible that the increase of one service may render another less profitable than it would otherwise have been. The proprietors of a railway with only one or two tracks may find that the increase of the goods traffic causes the passenger traffic to be attended with greater expense; the fuel of the company and the labour of its employees being wasted while the passenger trains have to wait in side tracks to avoid collisions.

It is very possible that both tendencies may be present, not coincidently, but with reference to a different extent of variation in the products under consideration. Thus a certain increase in the goods traffic by crowding the present line as above described might act in *rivalry* to the passenger traffic; but with reference to a large increase in the goods traffic, such as to make it profitable to have an additional track and so obtain the economies of production on a large scale, the goods traffic may be considered as *complementary* to the passenger traffic.* I do not pretend to discern to which of the two categories each concrete case belongs; I only wish to distinguish their properties in the abstract.

Among methods of governmental control, one of the most

* See Index, *sub voce* Joint Production.

important is that which consists in fixing a maximum tariff; provided that the maximum is not suspended on high, but is such as really to restrain the action of the monopolist; in which case its operation is nearly similar to that of a fixed tax. Suppose now that the price of one product is fixed, but not so that of another; or, what is more probable, that there is an effective maximum for one article and an inoperative maximum for another. The effect on the price of the second article will differ according as the products are *complementary* or *rival*. If they are complementary, the lowering of the price of the first is followed by the lowering of the price of the second; the benefit in respect of one commodity is a benefit also in respect of the other commodity. If the products are rival, there is a benefit to one class of consumers and a loss to another; provided, of course, that the loss to the monopolist is not so great as to induce him to give up the business.

The same rule applies to the effects of a law which requires that the price of an article in one market should not exceed its price in another by more than a certain percentage.[1] What is a benefit in respect of one commodity will be also a benefit in respect of the other, if the products are complementary; but a loss in the case of rival production.

A corresponding rule applies to a tax of the kind called "specific," that is, of so much per unit of commodity. The loss to one class of consumers will be a loss to the other class in case of complementary products; but a gain in the case of rival products.

The case of connected *demand* does not admit of equally definite rules. It is probable, but not certain, that the rules enounced for rival and complementary production hold good respectively for complementary and rival demand. Thus a maximum which lowers the rate for the terminal services of a railway tends probably to raise the rates for carriage since the demands for the two services are complementary. But a maximum which lowers the fare for third-class passengers tends probably to lower the fares for the first class, since the demands for the two kinds of tickets are rival.

The probability increases when the tendency of demand is in the same direction as that of production, and diminishes in the contrary case.

These propositions respecting the influence of demand may

[1] Generalising the conceptions of the American "*Short-haul Clause*," as it is commonly understood.

be applied to a law against differential charges and to a specific tax.

A tax proportional to the profits of the monopolist falls entirely upon him, as Cournot and Professor Marshall have proved. It should be added that a "progressive" tax on monopoly profit acts similarly.[1]

The effect of limiting the profit of the monopolist to a fixed amount is generally indeterminate. It may be advantageous or detrimental to some or all or none of the various groups of his customers. The fixing a (*bonâ fide*) maximum rate of profit on the capital expended acts to the advantage of the consumer.

I am not blind to the practical difficulties which stand in the way of a tax on the net profits of a monopolist, and of other measures that are here discussed. It cannot be too often repeated that the rules derived from mathematical reasoning are essentially abstract and require in practice to be largely diluted with common sense.

[1] Since this article was printed I have found that Knut Wicksell had preceded me in pointing this out.

## (F)

# PROFESSOR SELIGMAN ON THE THEORY OF MONOPOLY

[This article, which appeared in the Economic Journal, 1897, under the title "Professor Seligman on the Mathematical Method in Political Economy," might, in accordance with that title, equally well have been placed in the mathematical section of this Collection. The subject-matter is the theory of monopoly; the form is largely mathematical. The mathematical method is tested by an encounter with the classical method wielded by a powerful hand. The victory, if indeed it has been won, is of a somewhat Pyrrhic character. For it does not much redound to the credit of the mathematical method that the points in which it has an advantage are just those which have been neglected by an economist of conspicuous wisdom, one whose judgments on Income Taxes, Public Loans and other momentous interests are generally approved and followed. It is strongly suggested that matters which such an author did not take pains to state precisely cannot be of great importance. They might be compared to the points of detail on which the critical shoemaker corrected the masterpiece of the Grecian painter. Even without those little corrections the piece would no doubt have been a first-rate work of art. So the Art of Political Economy is not much affected by judgments on the question whether the taxation of a monopolised article is likely to be more or less burdensome to the consumer according as the production obeys the law of increasing or decreasing returns, or according as the demand is more or less elastic. Still, if such questions are posed, it is better not to answer them carelessly.

The criticisms on the original version having been directed against the second edition of Professor Seligman's *Shifting and Incidence*, published 1899, some of them have now been withdrawn or modified in deference to emendations in the third edition, published 1910.]

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I. (1) Following the order in which Professor Seligman has discussed the several issues, I notice first his objection to my

theorem that if a monopolist deals in two commodities for which there is a rival demand, such as first- and third-class service in a railway like our Midland with only those two classes of passenger fares, then if a tax is imposed on first class fares, it is theoretically possible that the monopolist proprietary of the railway may find it to their interest to *lower both* fares. Upon this Professor Seligman remarks :—

"That it [the mathematical method] sometimes leads to results which are likely to divorce still more the economics of the closet from the economics of the market-place, may be illustrated by a slip of Mr. Edgeworth himself. [See the extended mathematical proof (in the ECONOMIC JOURNAL, vii., pp. 230–232) of the proposition that a tax on first-class railroad tickets will reduce (not increase) the price of tickets of *all* classes]. The mathematics which can show that the result of a tax is to cheapen the untaxed as well as the taxed commodities will surely be a grateful boon to the perplexed and weary secretaries of the Treasury, and ministers of finance throughout the world." [1]

I hope that a sufficient reply will be made to these objections if ($\alpha$) I show that the proposition in question is agreeable to *a priori* presumptions, and ($\beta$) I verify it by a numerical example.

($\alpha$) The presumptions of the case are summed up in an answer which I received from an eminent economist—not specially devoted to the mathematical method—to whom I had submitted my little theorem, asking if it seemed to him too paradoxical to be credible. "I should hardly be surprised at anything in a regime of monopoly," was his reply. I might illustrate the paradoxical character of this regime by referring to one of its peculiarities which is relevant to the present question. If the demand for an article is *raised* [2] in the sense that more of it is demanded at each price than before; then, whereas in a regime of competition, *ceteris paribus*, theoretically in general the price will rise, this rule is not equally universal in a regime of monopoly : there the price may fall while the demand rises.[3]

The unexpected is all the more likely to occur in the case before us in that it is a case, not only of monopoly, but also of rival demand. Even in a regime of competition, as I have pointed out,[4] the taxation of articles for which the demand is *correlated* (either rival or complementary) is apt to produce curious results. *A*

[1] *Shifting and Incidence*, 2nd edition, p. 174.

[2] As to this technical use of the term rise of demand see Sidgwick's *Political Economy*, II. ch. ii. § 2, and *cp.* Marshall, *Principles of Economics*, Book III. ch. iii., Art. 4, Book V.

[3] See ECONOMIC JOURNAL, Vol. VII. p. 234.

[4] *Ibid.*, Vol. VII. p. 55.

*fortiori*, when the peculiarities of monopoly are combined with those of correlated demand. In the case of independent demand for a single article, in a regime of monopoly, the consumer may bear only a very small [1] proportion of the tax, even under the law of constant (not to say increasing) cost, under which the proposition would not be true in a regime of competition. When the circumstance of rival demand is superadded to monopoly, is it to be wondered at that the abnormality, as it appears in relation to the simpler case usually contemplated, should be increased: that the consumer should not only not be damnified, but should even be somewhat benefited by the tax?

A general idea of the modification due to the introduction of rival demand may be obtained by observing that alike in the case of independent and that of correlated demand, a tax on a monopolised article results in the diminution of the quantity of that article put on the market; [2] but, while in the case of independent demand the diminution of the quantity supplied is attended with a rise of price, this consequence does not necessarily follow in the case of rival demand. A tax on first-class fares results in the diminution of the quantity of first-class service supplied; and accordingly there is a flow of passengers from first class to third class. The demand for third-class service thus *rises* in the technical sense referred to in a former page, where it was stated that in monopoly this rise of demand may be attended with a fall of price.[3] The lowering of third-class fares results in a *fall* of the demand for first-class service in this technical sense, that for every possible first-class fare, the third-class fare being supposed constant at its new figure, the amount of first-class service demanded is less than what it would have been for each first-class fare before the disturbance, the third-class fare being supposed constant at its old figure. Now, it is quite consistent with ordinary presumptions that, when the demand for an article falls in this sense, its price should fall. Accordingly, the first-class, as well as the third-class fare, may be reduced.[4] *A fortiori*, it is possible that,

[1] The argument considered as *ad hominem* becomes *a fortiori*, since Professor Seligman thinks it possible that the consumer in this case may bear *no* part of the tax (*cp.* below, p. 161).

[2] I suppose that this proposition would be accepted by an opponent, as it is what may be expected from the analogy of competition, and is less than what those who trust that analogy accept. For a proof of the proposition I must refer to my article on " La Teoria pura del Monopolio " in the *Giornale degli Economisti* for 1897. [Above, E].

[3] Above, p. 144.

[4] It may assist conception to imagine first-class and third-class services controlled by different managers. The steps described in the text might be made by

though the first-class fare may be a little increased, yet the third-class fare may be so much diminished that the consumers as a whole may gain.

Considered as a mere possibility, this statement is not open to Professor Seligman's raillery in the passage above quoted. The plausibility of his objection is obtained by substituting a downright indicative—"the tax *will* reduce . . . the price" . . . "the result of the tax *is* to cheapen" . . .—for the potential mood in which I had couched my proposition, "a tax on one commodity *may* benefit the consumers of both . . . "the consequences of the new tax *may* be," . . . and so on.[1] I added the following caution :—

"Of course I do not suppose so delicate an adjustment—such a frictionless movement towards the position of maximum profit—to be realised in the concrete management of an English railway. But I think that it may be of scientific interest to establish the theoretic possibility of the paradox."[2]

The proposition in question is to be taken in the same spirit as the paradox of Mill, that an *improvement* in the production of an export may be *detrimental* to the exporting nation.[3] What should we think of a free trade writer who remarked on Mill's theorem that it would surely be a grateful boon to weary and perplexed ministers of commerce, since now all they had to do in order to foster commercial prosperity would be to injure the manufacture of exported commodities! Mill's theorem is useful as presenting an extreme and striking instance of a general truth which, if not indeed paradoxical, is yet not so familiar, but that it is desirable to call attention to it, the important truth that the interests of the parties to international trade are not so completely identical as some free traders have supposed. So our paradox calls attention to the truth that taxation in a regime of monopoly is more diversified and irregular in its consequences, and I think it may be added, likely to be less detrimental to the consumer, than an equal impost in a regime of competition. The extreme exemplifications of these truths are not designed to ease "perplexed and weary ministers," but to startle indolent and prejudiced

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the respective managers each endeavouring to maximise the net return in his department. But the further step, which on this supposition would be likely to occur, namely, the continued reduction of the first-class fares in order to steal custom from the third-class department, would be stopped by the directors of the common concern, who would not allow a gain to the first-class department to be purchased by a preponderant loss to the third-class department.

[1] Economic Journal, Vol. VII. pp. 230 and 232. [2] *Ibid.*, p. 231.

[3] *Political Economy*, Book III. ch. xviii. § 5.

economists from their dogmatic slumber, and incite them to reflect that maxims learnt too well from the study of familiar cases cannot always be applied without modification beyond the sphere of experience on which they were founded.[1]

These preliminary considerations will, I hope, dispose the student to attend to the mathematical ratiocination by which I have elsewhere deduced the theorem under consideration.[2] Addressing the general reader rather than the mathematician at present, I will not repeat this second part of the proof. I confine myself to the third stage, the verification, which consists in instancing laws of cost and of demand which actually fulfil the theory.

($\beta$) Let us put $p_1$ as the price of a first-class ticket for a certain journey, or number of miles, and $p_2$ as the price of a third-class ticket for the same journey. At these demand-prices let the number of the first-class passengers be $x$, that of the third-class passengers be $y$. Then, agreeably to the general laws of demand, $p_1$ must be so related to $x$ that, other things being the same, $p_1$ decreases as $x$ increases (and conversely); and $p_2$ must be similarly related to $y$. Also as first-class and third-class service are *rival* commodities, an increased supply of third-class service, while the amount supplied of first-class service remains constant, will be attended with a decrease in the first-class fare at which that amount of first-class accommodation is demanded. And the numbers of the first-class passengers will be similarly related to the third-class fares. These conditions, and any others that may reasonably be required,[3] are fulfilled

[1] On the meaning and use of *paradoxes*, compare De Quincey, *Words*, Ed. 1889, i. p. 199, and vii. p. 206. "Several great philosophers have published, under the idea and title of paradoxes, some first-rate truths, on which they desire to fix public attention, meaning, in a short-hand form, to say: 'Here, reader, are some extraordinary truths, looking so very like falsehoods, that you would never take them for anything else if you were not invited to give them a special examination.'"

[2] *Giornale degli Economisti*, 1897. [Above, E.]

[3] Professor Irving Fisher in his *Mathematical Investigations* has suggested the question whether the prices of two articles, $x$ and $y$, for which the demand is correlated, must be regarded as the partial differentials, with regard to $x$ and $y$ respectively, of a certain function which represents the total utility afforded by any quantities of $x$ and $y$. I answer this question in the affirmative (See *Giornale degli Economisti*, 1897, p. 314 note), with the same reservations as the conception of total utility requires in the case of a single variable, in particular that it should not be measured from the extreme point of privation: and accordingly take $p_1dx + p_2dy$ in my example as a complete differential of a function which I need not write out, but may call $U$. It may facilitate conception to consider the case in which $x$ and $y$ are not articles of consumption but factors of production; for instance, the carriage of different kinds of goods, for which $p_1$ and $p_2$ are the respective fares. Then $U$ may stand for the sum of the producers' surpluses

over a considerable range of prices[1] by the following laws of demand :—[2]

$$p_1 = £5 \times \left(1{\cdot}605\dot{3} - \frac{\cdot 2x}{20{,}000} - \frac{2}{3}\left(\frac{x - 19200}{20{,}000}\right)^{\frac{3}{2}} - \frac{1}{2}\frac{y}{100{,}000}\right)$$

$$p_2 = £1 \times \left(3{\cdot}9\dot{1}\dot{8} - 2\left(\frac{y - 69750}{100{,}000}\right)^{\frac{1}{2}} - \frac{1}{2}\frac{x}{20{,}000}\right)$$

We may begin by supposing the cost constant—a very possible case, as Cournot has remarked.[3] Then the profit, which it is the object of the monopolist to maximise, the net monopoly revenue, in the phrase of Professor Marshall, is of the form $x \times p_1 + y \times p_2 - C$, where $C$ is a constant. It may be shown first that this net profit is a maximum, when $x = 20{,}000$, $y = 100{,}000$ corresponding to the fares $p_1 = £5 \times \cdot 9 = £4$ 10*s.*; $p_2 = £1 \times 2{\cdot}3\dot{1}\dot{8} = £2$ 6*s.* $4{\cdot}\dot{3}\dot{6}$*d.*; secondly, that if there is imposed on first class travelling any tax of so much per ticket, not exceeding $\cdot 16885$ £5, or about 16*s.* 10½*d.* per ticket, it will become the interest of the monopolistic company to *lower* the fares *both* for first-class and third-class passengers.

The statement may be conveniently altered by putting $\xi$ for $x \div 20{,}000$, and $\eta$ for $y \div 100{,}000$. Then the expression which is to be maximised becomes $20{,}000\xi \times 5(1{\cdot}605\dot{3} - \cdot 2\xi - \frac{2}{3}(\xi - \cdot 96)^{\frac{3}{2}} - \frac{1}{2}\eta) + 100{,}000\eta\ (3{\cdot}9\dot{1}\dot{8} - 2(\eta - \cdot 6975)^{\frac{1}{2}} - \frac{1}{2}\xi)$ (+ a constant). And it has first to be shown that this expression is a maximum when $\xi = 1$ and $\eta = 1$. The reader to whom this sort of investigation is not familiar may be advised to substitute in (the variable part of) the above-written expression values for $\xi$ and $\eta$ at first very close to unity, *e. g.* for $\xi$, 1·001, or 1 — ·001, and for $\eta$ values about equally distant from unity; then gradually enlarging the divergence from unity to realise that for a considerable distance on both sides of unity the result of substituting different values of $\xi$ and $\eta$ is to make the expression smaller than what it is for $\xi = 1$ and $\eta = 1$: that is, $3{\cdot}2\dot{1}\dot{8} \times 100{,}000$.[4]

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enjoyed by the customers of the railway on the assumption that each producer will push his expenditure on each factor of production up to the margin of profitableness.

[1] As to the range over which the formulæ are applicable, see below, p. 149, note [and ECONOMIC JOURNAL, Vol. XX. p. 291, in a note not reprinted here for a reason mentioned in the Introduction].

[2] From these simultaneous equations we can obtain $x$ and $y$ in terms of $p_1$ and $p_2$; as stated ECONOMIC JOURNAL, 1897, p. 54.

[3] *Principes Mathématiques*, Art. 30.

[4] If $\Delta\xi$ be the difference between the assumed value of $\xi$ and unity, and $\Delta\eta$ the difference between the assumed value of $\eta$ and unity, then, $\Delta\xi$ and $\Delta\eta$ being small, the increment of the monopolist's net profit consequent upon the change from $\xi$

Consider next the consequence of imposing a moderate tax of so much per ticket on first-class fares, say, £5 $\times \tau$, where $\tau$ is a fraction not exceeding ·16885. The amount to be maximised is no longer now $xp_1 + yp_2$, but the same $- 5\tau x$; that is, if we employ $\xi$ and $\eta$ as before, the same expression as before, *minus* $100{,}000 \times \xi\tau$. The value of this modified expression when $\xi = 1$ and $\eta = 1$ is $100{,}000\ (3\cdot2\dot{1}\dot{8} - \tau)$. It will be found that for any assigned value of $\tau$ (up to the limit mentioned), there can be found a value of $\xi$ less than unity, and a value of $\eta$ greater than unity, such that the monopolist's revenue, as modified by the tax, should be a maximum for those values, while *both* the prices—both the first-class and second-class fares—are *less* than what they were before the imposition of the tax.

Here, as before, the reader may be advised to begin with small quantities. To any small value of $\tau$ there may be expected to correspond two values of $\xi$ and $\eta$ in the neighbourhood of unity, rendering the (modified) monopolist revenue a maximum. For example, to $\tau = \cdot0017155\cdots \times$ £5, a little more than twopence per ticket, there correspond the values $\xi = \cdot999$, $\eta = 1\cdot0015845231\cdots$; and it may be found by actual substitution, or better by general reasoning,[1] that the loss to the monopolist through the decrease of his receipts is ·000,0009 × £100,000 nearly, while his gain in having to pay tax on a smaller number of first-class tickets is double that amount, viz. ·000,0017 × £100,000 nearly. The monopolist is, therefore, better off with

and $\eta$ is approximately $-\frac{1}{2}(3\cdot3\Delta\xi^2 + 2\Delta\xi\Delta\eta + \cdot6311\Delta\eta^2)$; as may be shown by expanding the surd or irrational terms in the expression for the profit according to the *algebraic* rule for extracting the square root (*cf.* Todhunter's *Algebra for Beginners*, ch. 28), and neglecting powers of $\Delta\xi$ and $\Delta\eta$ above the second. The above expression for the increase of the profit is negative, whatever the *signs* of $\Delta\xi$ and $\Delta\eta$: showing that the profit corresponding to $\xi = 1$, $\eta = 1$ is greater than for any other values in the immediate neighbourhood. In whatever direction we step from the position defined by the equality of $\xi$ and of $\eta$ to unity, we descend and continue to descend to a considerable distance in every direction—for instance up to $\xi = \cdot96$, $\eta$ remaining 1, up to $\eta = \cdot6975$, $\xi$ remaining 1, and much further in the *positive* directions of $\xi$ and $\eta$. The stoppage at those points has, of course, no economic significance: it was adopted merely for the sake of arithmetical convenience; otherwise it would have been better to use cube roots where now square roots are used.

[1] The clue to the investigation is given by the following equations. [See E, p. 127.] Let the new $\xi$ (corresponding to the maximum after the tax) be $1 + \Delta\xi$ (where $\Delta\xi$ is a small quantity positive or negative), and similarly let the new $\eta$ be $1 + \Delta\eta$. Then the values of $\Delta\xi$ and $\Delta\eta$ in term of $\tau$ are given by the following (simultaneous) equations

$$\begin{cases} \Delta\eta + 3\cdot3\Delta\xi + \tau = 0. \\ \cdot6311\Delta\eta + \Delta\xi = 0. \end{cases}$$

These equations are approximately satisfied by $\Delta x = -\ \cdot5829\tau$, $\Delta y = \tau/108263$, They become less and less exact as $\Delta\xi$ and $\Delta\eta$ are increased, with the increase of $\tau$.

the new values of $\xi$ and $\eta$ than he would be (after the imposition of the tax) with the old values, and, as it will be found, with any other values of $\xi$ and $\eta$. But these values of $\xi$ and $\eta$ correspond to *lower* values of $p_1$ and $p_2$ (first- and third-class fares) than existed before the tax; as may be seen if these values are substituted for $\xi$ and $\eta$ in the expressions for $p_1$ and $p_2$ respectively.

As we increase $\tau$ and therewith decrease $\xi$ and increase $\eta$, the general relations which have been indicated persist: the monopolist gains more by escaping part of the tax than he loses by the diminution of the receipts, as the values of $\xi$ and $\eta$ move further away from unity (the proportion of the gain to the loss becoming greater as the absolute quantities become greater), while both the fares continue to diminish. Thus for the limiting value of $\tau$, viz., $\cdot16885\cdots$ we have $\Delta\xi = -\cdot04$ and $\Delta\eta = \cdot05248$. And by actually substituting $\cdot96$ for $\xi$, and $1\cdot05248$ for $\eta$, it is found that the new receipts are less than the old receipts by about $\cdot002 \times$ £100,000. Against this loss is to be set off the gain of saving the tax of £5 $\times \cdot16885$ on $\cdot04 \times 20{,}000$ first class tickets: that is a gain of $\cdot00675\cdots \times$ £100,000—a gain more than three times greater than the loss. At the same time the first-class fare is diminished by $(\frac{1}{2}\ \cdot05248 - \cdot2 \times \cdot04 - \frac{2}{3}\cdot008) \times$ £5, that is, diminished by $\cdot0129 \times$ £5 nearly, $=$ 1*s*. 3½*d*.; and the third-class fare is diminished by £2 $(\sqrt{\cdot3025 + \cdot05248} - \cdot55) - \frac{1}{2}\cdot04$, $=$ £$\cdot0716$, $=$ 1*s*. 5*d*. nearly.

The net gain of some £475, which we have found to attend the lowering of both fares, might well be a substantial percentage of the net profits, supposing these to be, say, about 7 per cent. of the gross profits, which were originally £$321{,}818\cdot\dot{1}\dot{8}$. Say the net profits (per month or year) are about £20,000, the monopolist gains about 2 per cent. on his net profits by making the adjustment described.

We have so far been supposing the total cost to be a fixed amount, say about £300,000. But the reasoning is not materially altered when we suppose the cost variable. To take a simple instance, let the cost consist partly of a constant sum, and partly of two additional amounts respectively proportional to $x$ and $y$, the number of first-class and that of third-class tickets. To obtain now the expression for the monopolist's net revenue we must deduct from the gross receipts $xp_1 + yp_2$, the cost $xk_1 + yk_2$, where $k_1$ and $k_2$ are the cost per unit of $x$ and $y$ respectively. Much the same conclusion as before may be brought out if we put $p'_1 = p_1 + k_1$, $p'_2 = p_2 + k_2$; the expression which the monopolist seeks to maximise becoming now $xp'_1 + yp'_2$.

One consequence of admitting the variation of the cost is to render the occurrence of anomalies more probable. When correlation of cost is superadded to correlation of demand, and both to monopoly, we may look out for freaks in the incidence of taxation.*

I should be curious to know what " slip " has been detected in this reasoning, substantially identical with what has been already published.[1] In a matter of this sort imputations of error based upon first appearances should be objected sparingly.

(2) I take next Professor Seligman's theory as to the relation between the law of cost and the pressure of taxation.**

The debate on this matter has been somewhat embarrassed by the disputants having used the principal terms in different senses. According to the definition which I have employed in

* It will be shown on a later page (II. 124) that even in the regime of Competition, when both cost and demand are correlated, a tax on one of the articles may cause the price of *both* to fall. *A fortiori* of course in the regime of Monopoly.

[1] The example given in my article in the *Giornale degli Economisti*, 1897 [above, E], differs only in details from the one here given.

** There is here omitted the statement of a thesis maintained by Professor Seligman in the *second* edition of his *Shifting and Incidence*, viz.: " Under ordinary conditions, therefore, in the case of a tax on monopolistic industry subject to the law of increasing returns or diminishing cost, the tendency is that the consumer will suffer less than in the case of an industry subject to the law of constant cost. . . . On the other hand, if the monopoly obeys the law of diminishing returns or increasing cost, the producer will be likely to add more of the tax to the price than in the case of constant or increasing returns." This thesis being no longer maintained by the author (*third* edition, p. 248, note), I have withdrawn the lengthy dialectic which in the original article was directed against certain arguments used in the *second* edition in support of the thesis as stated there. But it is not necessary to withdraw all the negative reasoning in this context. The two paragraphs retained at the end of the section headed (2) are applicable, with a slight alteration, to the thesis of the *third* edition; which may be described in logical terms as the *contrary*, whereas it ought to have been only the *contradictory*, of the original thesis. According to the new thesis, " if a monopolised article is the product of an industry which obeys the law of increasing return or diminishing cost," then, " as in the case of competition, he [the monopolist] will add more of the tax to the price. *Vice versa* under conditions of diminishing return, or increasing cost, the monopolist producer will be likely to add less of the tax to the price than in the case of constant return " (*Shifting and Incidence, third* edition, p. 247). It will be here maintained that the true proposition respecting increasing and diminishing returns in the second of the senses above distinguished has by no means the universality expressed by the above statement. The positive portion of section (2) is retained partly as possessing, it may be hoped, some intrinsic interest, partly as leading up to the retained portion of the negative reasoning, and partly as relevant to the issue between diagram and algebra referred to on a later page (167). A glance at the formula there given reveals more quickly than does a study of the diagrams in this context the truth that the law of " returns " in our sense of the term does, and in the other sense does not, constitute an index of the pressure on the consumer due to an assigned tax.

the articles referred to, the law of increasing cost, synonymous with diminishing returns, holds good when the total cost of producing the quantity $x$ of a certain commodity increases with the increase of $x$ at an increasing rate; the law of diminishing cost, synonymous with increasing returns, holds good when the total expense of producing the quantity $x$ increases with the increase of $x$ at a diminishing rate.* In other words, the law of increasing cost, or diminishing returns, holds good when the ratio of the last increment of cost to the last increment of produce is greater than the ratio of the penultimate increment of cost to the penultimate increment of produce; with a corresponding statement for the law of diminishing cost (or increasing returns). "This definition," I intimated, "is not identical with that of some distinguished economists"; who may seem to compare the ratio of the last increment of cost to the last increment of produce, not with the ratio above stated, but with the ratio of the total expense to the total produce $x$, and may accordingly [1] define that the law of increasing or diminishing cost holds good according as the ratio of total cost to total product increases or diminishes with the increase of product.[2]

A geometrical illustration may put the matter in a clearer light.

In the accompanying diagram the ordinate $Y$ of the curve $O_1Q$ represents the total expense required to produce any amount, $x$, of a certain commodity, represented by the corresponding co-ordinate. The case represented is one in which a certain amount of expense, $OO_1$, must be incurred before any return at all is obtained. According to my definition—No. 1, we may call it—the law of increasing cost, or diminishing returns, holds good for all tracts of a curve oft his sort which are *convex* to the axis of $X$, that is, in the case illustrated, throughout. According to the other definition, No. 2, the law of increasing cost holds good only for those tracts for which the slope of the curve, the inclination of a tangent at any point of the curve to the axis of $X$, is greater than the inclination to that axis of a line joining that point to $O$. In other words, according to definition No. 2, the law of increasing cost holds good while the ratio of total cost to produce increases

* Above, E, p. 67 *et seq.*

[1] If $x$ is the quantity produced and $f(x)$ the corresponding total cost, it comes to much the same whether we take as the essential attribute of increasing cost the fact that $f'(x)$ is greater than $f(x) \div x$, or that $f(x) \div x$ increases as $x$ increases.

[2] There may be other shades of meaning, especially in the case of competition as distinguished from monopoly. The difficulties presented by "increasing returns" in a regime of competition are noticed in one of the articles referred to (S, II. 87 [*cp.* ξ]).

with the increase of produce. The relation between the two definitions is illustrated by the diagram. Beyond the point $Q$, at which a tangent to the curve passes through $O$, the law of increasing cost holds good in both senses; but on the near side of $Q$ there is increasing cost in the first sense, but diminishing cost in the second sense. If the origin had been at $O_1$, the axis of $X$ being a horizontal through that point, then the law of increasing returns would prevail *throughout* in the second sense as well as in the first sense. If any cost-curve possess either of the attributes continuously *ab initio* in one sense, then it will possess that attribute in the other sense also throughout.

The same diagram (which may with advantage be viewed from behind the paper) can be used to illustrate the different definitions

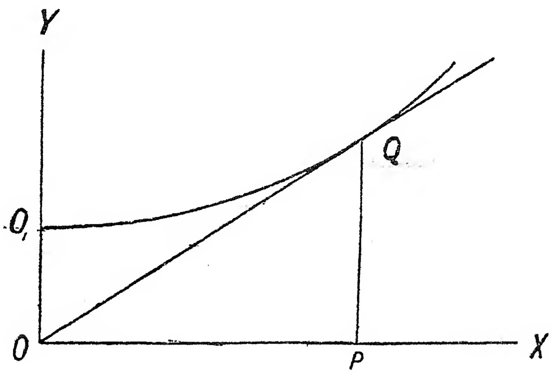


FIG. 1.

of the laws, if the axis of $Y$ denotes produce, and the axis of $X$ corresponding cost. The curve now represents a case in which a certain amount of produce, $OO_1$, is given by nature without any outlay. For the tract $O_1Q$, up to the limit where a tangent from the origin touches the given curve, the law of increasing cost prevails according to the second definition, the law of diminishing cost according to the first definition; after the limit $Q$, the law of diminishing costs in both senses.

Now as to the sense in which Professor Seligman uses the terms, the first definition is suggested by the following passage :—

"If . . . an industry obeys the law of increasing returns or diminishing cost—where each increment in the amount produced costs less than the last" (*Shifting and Incidence*, Second Edition, p. 205).

But the context shows that the second, not the first, definition is in his mind : —

"If he produces less, each unit will, on the supposition that

he has been producing under conditions of increasing returns cost him, exclusive of the tax, more than before." (*Ibid.*)

Similarly, the statement that, under the condition of diminishing returns, " each additional increment of production costs more than the last," is explained away by the context. The author's diagrams (*op. cit.*, pp. 209–210) leave no doubt as to his use of the terms. It is the cost per unit which he takes as increasing or diminishing with the law of increasing or diminishing cost. Compare his frequent use of the phrase " ratio of product to cost " (pp. 192, 211, 273, 278).

I do not complain of his employing the terms in a sense which is both useful and usual. All that I am concerned to maintain is (*a*) that my proposition in my sense of the terms is true, and (*b*), that his proposition in his sense of the terms is not so.*

(*a*) We may follow Professor Seligman in first supposing the law of constant cost to prevail, and afterwards substituting the law of increasing and diminishing returns respectively, other things being (as much as possible) unchanged. Only, with reference to definition I, " constant cost " must be interpreted to mean, not that the cost per unit is constant, but that the increment of the total cost per increment of product is constant; in other words, that the total cost curve is a right line, but not necessarily a right line passing through the origin, as definition II requires.

In the annexed, as in the former diagram, let the axis $X$ represent total produce, and let the total cost curve, illustrated by the former diagram, pass through $Q$. Let the curve $Ss$ represent, by its ordinate, the gross receipts (product $\times$ price) corresponding to any value of the product $X$. The position of maximum advantage to the monopolist is where the difference between the gross receipts and the total cost is a maximum: that is, at a point where the tangent to the cost curve is parallel to the tangent at the corresponding point of the gross receipts curve.[1] Thus, if the total cost curve is a right line $BB'$ passing through $Q$ (vertically above $P$) in order that $OP$ should be the amount supplied, the line must be parallel to the tangent at $S$ (vertically above $Q$ and $P$) to the curve $Ss$.

* These words may still stand, although the proposition, occurring third in the edition of *Shifting and Incidence* which is now to be disputed, is not the same as that which, occurring in the *second* edition of that work, was disputed in the original article.

[1] Compare the illustration given III. 92, noticing that the curve $BB'$ there represents net profits, not as $Ss$, here gross receipts. In order that there should be a maximum (the law of constant cost prevailing) the curve $Ss$ must be *concave* towards the axis $OX$.

Now let us introduce successively the incidents of increasing and of diminishing cost; neither altering the law of demand, nor the amount of the total cost nor that of the cost per unit at the point $P$, and accordingly neither altering the price, nor the amount supplied, $OP$.[1]

It is evident that these conditions can only be fulfilled by a curve ($AA'$) convex to the axis of $X$, and a curve ($CC'$) concave to it (corresponding respectively to increasing and diminishing cost in my sense of the terms), each curve touching the line ($BB'$) at the point $Q$.

Now let a tax of so much per unit be imposed on the mono-

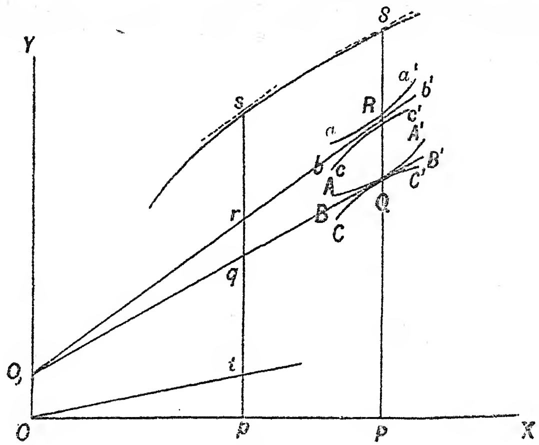


FIG. 2.

polised article. The effect of the tax is to push up every point on the total cost curve to an extent which is determined by the following construction :—[2]

Through $O$ draw the right line $Ot$, making an angle with the axis of $X$ such that at any point on the line, $t$, the perpendicular $tp$ may represent the (total) tax paid on the product $Op$.[3] To find the displacement of any point, $q$, on the cost curve, draw a vertical line through $q$ and measure upwards $qr$ equal to $pt$ intercepted between the lines $OP$ and $Ot$. This construction holds for the curves of varying as well as for that of constant cost. Accordingly the new laws of cost formed by superadding the tax to the old cost will be related as shown in the diagram. The representation of constant cost will still be a right line ($bb'$), only inclined at a

[1] See the remarks on p. 159, par. 3, below.

[2] Compare Messrs. Auspitz and Lieben's construction for the representation of a specific tax.

[3] The tax $tp$ = the product $Op \times$ the tangent of the angle $pOt$.

greater angle to the axis of $X$ than the old line ($BB'$). The law of increasing cost will still be represented by a curve ($aa'$) convex to the axis of $X$, the new curve touching the new right line at $R$. The law of diminishing cost ($cc'$) will similarly retain, after the increase of cost by the last, both the character of concavity and the incident of contact with the right line representing constant cost.

Let us now compare the additions to the price consequent on the change of cost in each of the three cases. In the case of constant returns we have by construction the slope of $bb'$ greater than that of $BB'$, the slope of $BB'$ equal to that of the tangent at the point $S$ to the curve $Ss$, the slope of the tangent to this curve increasing as we move towards $O$, and diminishing as we move from it.[1] Therefore, to find the point at which the slope of the line $bb'$ is the same as the slope of the tangent to the curve $Ss$ at the corresponding point, we must move towards $O$, diminishing $OP$, say to $Op$, at which point the monopolist's profit is a maximum for the new law of cost. In the case of increasing cost the initial slope at $R$ is the same as that of the line $bb'$. Therefore, by a parity of reasoning, we must move to the left in order to reach a point at which the slope of the cost curve may be the same as that of the gross receipts curve. But as we move to the left, whereas the slope of the right line remains constant, the slope of the convex cost curve diminishes. Accordingly the point at which the slope of the cost curve becomes equal to the slope of the gross receipts curve will be sooner reached in the case of the convex cost curve than in the case of the right line representing constant cost. By a parity of reasoning the maximum point will be later reached in the case of the concave cost curve. That is to say, the diminution of the supply will be less, and therefore the addition to the price less for the law of diminishing returns, corresponding to the convex curve, than what it is for the law of increasing returns, corresponding to the concave curve. *Quod erat demonstrandum.*

(*b*) Now let us consider Professor Seligman's proposition in his sense of the terms. To show the irrelevance of the distinction between increasing and diminishing returns according to the definition adopted by our author, suppose that, other things remaining constant, the total cost is varied by taking the origin now at a point above $O_1$, now at a point below it, *e. g.* $O$. The more or less of rise in price still goes with the concavity or convexity of the cost curve, that is, with increasing or diminishing returns in my sense; the rise is neither more nor less whether the

[1] See note to p. 154.

new origin is above $O_1$ or below it, whether it is a case of diminishing or of increasing returns in Professor Seligman's sense. My proposition remains true in whatever light contemplated; his proposition changes its quality with the accident of the theorist's point of view.

It would however be inaccurate to describe the law of increasing (or diminishing) returns in the second, the author's, sense as having no relation to the greater or less pressure of a tax. For there is a certain affinity between increasing (or diminishing) returns in the two senses. If the law of increasing (or diminishing) returns is fulfilled in the first sense continuously from the zero of production onwards, then it will be fulfilled in the second sense also.[1] Even if the curve of total cost has not been concave or convex *ab initio*, yet if it afterwards becomes so and remains so continuously, or at least for long tracts, then it is likely that many parts of the curve which fulfil one of the laws in the first sense will fulfil it also in the second sense. The very fact that the terms have been used by eminent economists in both senses without distinction forms a presumption that there is some affinity. In virtue of this affinity there is some connection between each of the laws in our author's sense and the property which he connects with it. There is a certain correlation; but not the coexistence which this statement implies.*

(3) The next question is, whether the tax of a monopolised article is more or less likely to hurt the consumer, according as the elasticity of demand is greater.** The subject is thus introduced by Professor Seligman; apparently without distinction in this first statement between competition and monopoly.

"If, thirdly, the demand is elastic as in the case of minor luxuries and of all comforts, that is of the general mass of commodities . . . the tax will be divided between the consumer and the producer. The proportions in which this division will take place will depend, so far as this element is concerned, chiefly on the elasticity of the demand. The more persistent the demand, the greater is the proportion of the tax which the producer will be able to add to the price; the more sensitive the demand, the smaller the sum by which he will find it profitable to increase the

[1] *Cp.* above, p. 70.

* This sentence has been substituted for the more damnatory verdict referring to the Second Edition.

** I use the term "elasticity" in this context in the same sense as the author whom I am criticising; that is, as I have pointed out (ζ, II. 394), not the received exact sense, but accurate enough for most of the reasoning here. Where any difference would be caused by the use of the proper definition I employ "elasticity" in inverted commas to denote the popular, Professor Seligman's, conception.

price. In other words, the greater the elasticity of the demand, the more favourable—other things being equal—will be the situation of the consumer " (p. 191).*

In a later passage, connected with the above by a footnote, it is written :—

" If the demand falls off greatly with every increase of price—or, in other words, if the margin of effective demand is small—the price, as we have seen, will be increased by much less than the amount of the tax, and the producer will suffer most of the loss. Conversely, if the demand is not so elastic—if an increase of price will produce only a small decrease of demand—a larger proportion of the tax will be added, and the consumer will suffer more than the producer " (p. 204).

In this passage the author is referring specially to " a monopolised industry," subject to the law of constant returns. He goes on to consider the cases of increasing and diminishing returns, both in monopoly and competition, concluding—

" In all these cases—whether of competition or of monopoly, of increasing or of diminishing cost—the important point remains, as before, the elasticity of demand " (p. 208).

The passages, as I interpret, contain rather a statement than a demonstration of the author's theory respecting the influence of greater or less elasticity. For what is called " a formal proof " we are referred, in the last but one of the passages above quoted, to the following passage :—

" It was stated above " [referring to the passage at p. 204, which I have already transcribed] " that the more elastic the demand, the smaller the proportion of the tax that he (the monopolist) would be apt to shift to the consumer. It may be wise to illustrate this also by some simple arithmetical figures " (p. 277).

As these figures are from an example given on the immediately preceding page, it may be well, parenthetically, to explain that in that example there was supposed a law of demand such that there was demanded :—

| | | | |
|---|---|---|---|
| At price | \$5 | ........ | 1000 units of commodity. |
| ,, | $5\frac{1}{4}$ | ........ | 900 ,, ,, |
| ,, | $5\frac{1}{2}$ | ........ | 825 ,, ,, |
| ,, | $5\frac{3}{4}$ | ........ | 750 ,, ,, |
| ,, | 6 | ........ | 700 ,, ,, |

The cost is supposed to be constant, viz. \$2 per unit.

* The reference in this and following passages is to the *second* edition of *Shifting and Incidence.* There seems no objection to retaining the references as given in my original article relating to theories which the author still maintains, as intimated at page 248 of his *third* edition. I advert to the third edition in a later article designated $\xi$ in the present Collection.

The author continues :—

"Demand is said to be more elastic when each successive increase of price leads to a greater falling off of demand. The example above was based on the assumption that, at the price of \$6, the demand would fall to 700. Let us now assume that, with a more elastic demand, the sales at price \$6 would fall off as far as 675 units; and let us further assume that, with a more stable demand, the sales at the price \$6 would fall off only to 725 units. Now, with the more elastic demand, the net profits would be, after the tax of \$1 per unit was imposed, $(6-3) \times 675 =$ \$2025; but with the less elastic or more stable demand the net profits would be $(6-3)$ \$725 = \$2175. Hence the more stable the demand the greater the chances of his increasing the price by the whole tax."

[The author had shown on a preceding page that if a tax of \$1 per unit is imposed the price will be raised by \$1 "the entire tax will be shifted to the consumer."]

Here, as in the case of the former issue, one must admire the true logical instinct which leads the inquirer to introduce the attribute under consideration, increase of elasticity, other things being preserved the same, in order to observe the effect due to the introduced attribute. But, as writers on logic have pointed out, the *method of difference* is not wholly empirical: it often requires a good deal of *a priori* knowledge obtained by deduction, in order to divine not only what "other things" may be allowed to vary as being immaterial to the matter in hand, but also what other things ought *not* to be kept the same. Suppose it to be inquired whether youths at a certain age, say between thirteen and fifteen, increased in weight with a particular degree of rapidity. In comparing the weights of youths at the two ages, it would be proper to keep some things the same—the persons themselves, for example. But it would not be proper to keep constant the *heights* of the youths under observation: it would not be proper to select instances in which there was no growth in height, in order to test whether in general there is a considerable growth in weight. For it is knowable *a priori* that there exists *correlation* between height and weight. The specimens selected for their constancy in height are not fair specimens of the change in weight. Now Professor Seligman selects his specimen with an analogous partiality. In order to test the effect of increased elasticity on taxation he had to construct a new law of demand, more elastic than the original one. He is within his rights in supposing, as he does in effect, that the new demand curve passes through the point which corresponds to the old price, the price

before the change of elasticity and before the tax. But he is not within his right when he in effect takes as a type on which to build a general argument a law of demand equivalent to a curve which not only passes through the point corresponding to the old price, but also is coincident with the old demand curve, for a considerable tract of price, for nearly the whole extent by which the price (before the change of elasticity) was pushed up by the tax. For, in general, the new demand curve ought to be conceived as *cutting* the old one at the point specified. Whence it is deducible that in general, when the law of elasticity is thus raised, there will be some change of price *before the tax*. This preliminary change is the attribute which corresponds to *height* in my apologue. The author has selected an instance which may be seen *a priori* to be favourable to his conclusion. An instance selected impartially at random, so to speak, would be very unlikely to present the phenomenon of both price and quantity unchanged by the change of elasticity. In fact did it indeed ever occur to any one, wishing to illustrate what Mill calls the increase of the demand in a greater or a less proportion than the cheapness,[1] to conceive a variant so peculiar as that which forms Professor Seligman's illustration?

The following is a more typical example. Let the law of demand be originally such that there is demanded:—

| | | | | |
|---|---|---|---|---|
| At price, $5 | ........ | 1000 | units of | commodity. |
| ,, $5\frac{1}{4}$ | ........ | 900 | ,, | ,, |
| ,, $5\frac{1}{2}$ | ........ | 800 | ,, | ,, |
| ,, $5\frac{3}{4}$ | ........ | 650 | ,, | ,, |
| ,, 6 | ........ | 475 | ,, | ,, |

the cost being, as before, constant, $2 per unit of commodity. Then, as in Professor Seligman's example, originally the monopolist will fix the price at $5. The net profit at that price, as shown in the accompanying statement, will be higher than at any other of the prices.

BEFORE THE CHANGE OF ELASTICITY.

| | | | | | | |
|---|---|---|---|---|---|---|
| | Output ........... | 1000 | 900 | 800 | 650 | 475 |
| | Price .................. | 5 | 5·25 | 5·5 | 5·75 | 6 |
| Before the tax. | Price *minus* cost ... | 3 | 3·25 | 3·5 | 3·75 | 4 |
| | Net profits ......... | 3000 | 2925 | 2800 | 2437·5 | 1900 |
| After the tax. | Price *minus* cost ... | 0·3 | 0·55 | 0·8 | 1·05 | 1·3 |
| | Net profits ......... | 300 | 495 | 640 | 682·5 | 617 |

[1] *Political Economy*, III. ch. xviii. § 5.

AFTER THE CHANGE OF ELASTICITY.

| | | | | | | |
|---|---|---|---|---|---|---|
| | Output ............... | 1000 | 900 | 800 | 650 | 475 |
| | Price .................. | 5 | 5,775 | 6·05 | 6·325 | 6·6 |
| Before the tax. | Price *minus* cost ... | 3 | 3·775 | 4·05 | 4·325 | 4·6 |
| | Net profits ......... | 3000 | 3397·5 | 3240 | 2811·25 | 2185 |
| After the tax. | Price *minus* cost ... | 0·3 | 1·075 | 1·35 | 1·625 | 1·9 |
| | Net profits ......... | 300 | 967·5 | 1080 | 1056·2 | 902·5 |

Now let a tax of \$2·7 per unit be imposed. The resulting figures given in the accompanying statement show, as far as a discontinuous schedule of this sort can show, that the price will now be raised from 5 to $5\frac{3}{4}$, that is by $\$\frac{3}{4}$ in consequence of the tax. Next let us introduce the circumstance of diminished elasticity. Understanding that after, as before the change of elasticity, 1000 units are demanded at the price 5, let us suppose the demand to be tilted up for each higher price in that neighbourhood. For example, whereas originally an output of 900 was carried off by a price of $5\frac{1}{4}$, let that price now become $5\frac{1}{4} \times 1{\cdot}1$, and let the other prices be increased in the same proportion. Then we shall have, after the change of elasticity, the prices specified in the second part of the annexed statement, corresponding to the original outputs. That is before the tax by the mere fact of diminished elasticity the price has been raised from 5 to 5·775. Now superadd the tax of 2·7 per unit, as before, and it will appear that the rise of price is from \$5·775 to \$6·05, that is, \$·275, considerably *less* than the rise of price under the condition of greater elasticity, which was ·75.

But of course this manipulation of figures is "mere palpation," affording no certain warranty of general truth, and rather calculated to obscure essential points, as in the memorable instance of an arithmetical example constructed by J. S. Mill and condemned by Professor Marshall.[1] A hundred, perhaps a thousand, empirical instances, taken impartially at random, might be required in order to elicit by a laborious elimination of chance the tendencies which may be discovered at a glance upon inspection of a few symbols.

(4) The next question is whether in general, or with what degree of generality, when a specific tax is imposed on a monopolised article, the price will rise. Professor Seligman now admits "that in ordinary cases the monopolist will shift at least a part of the burden." But he ventures "still to cling to the position" that "cases may arise in which it will be profitable for the monopolist to bear the burden himself. No part of the tax will be shifted to the consumer." He disputes my position that the tax

[1] *Principles of Economics*, Book VI. ch. ix., note on Ricardo's doctrine.

will affect the consumer, except "in two special cases, (1) where it is not in the power of the monopolist to increase or limit his output at will; (2) where the monopolist is a sole *buyer*, and the supply of the article bought is perfectly inelastic; for instance, a combination of tenants dealing with landlords [1] incapable of combining." [2]

"That these are not the only cases, however," remarks Professor Seligman, "is clear from the argument in the text." Here is this argument :—It is supposed, as in the example quoted on our page, that the monopolist will sell 1000 units at price \$5, 900 units at price \$5¼, and so on as stated in the third column of the annexed schedule. The cost per unit is supposed to be constant, viz. \$2. Professor Seligman argues :

"His net profits then after a tax of ¼ of a dollar had been imposed, would be,"

| | | | | | |
|---|---|---|---|---|---|
| At price | 5 | ... | $(5 - 2\frac{1}{4}) \times 1000$ | ... | = \$2750. |
| ,, | 5¼ | ... | $(5\frac{1}{4} - 2\frac{1}{4}) \times 900$ | ... | = \$2700. |
| ,, | 5½ | ... | $(5\frac{1}{2} - 2\frac{1}{4}) \times 825$ | ... | = \$2681·25. |
| ,, | 5¾ | ... | $(5\frac{3}{4} - 2\frac{1}{4}) \times 750$ | ... | = \$2625. |
| ,, | 6 | ... | $(6 - 2\frac{1}{4}) \times 700$ | ... | = \$2625. |

"In other words the monopolist will continue to find his greatest profits in continuing to charge the original price."

He will, I rejoin, if he can only alter the price *per saltum*, by leaps of ¼ dollar. But surely it was not necessary in an article on *pure theory* to notice this obvious limitation, which may fairly be relegated to the category of *friction*. If the monopolist can adopt an intermediate price between \$5 and \$5¼, I submit that he will tend theoretically to do so, for the reasons which I have given in one of the papers referred to.[3] Actually no doubt he may not do so, because the gain directly resulting from the change may not compensate the incidental disadvantages attending a change. This force of friction is well described by Professor Seligman, and is all the more clearly discerned by the mathematical economist, in that he perceives, as pointed out by Professor Knut Wicksell,[4] that when the tax is small the gain must be *very* small, of the second order.[5]

[1] *Pure Theory of Taxation*, S, II. 91.

[2] Assuming, of course, that the landlords have nothing else to do with their land but to offer it to tenants. Professor Seligman seems not to accept this postulate. He says, "the landowner is not compelled to part with his land; but the tenant is compelled to occupy some apartments" (*op. cit.*, p. 242).

[3] See especially the diagrammatic statement in the review of Professor Graziani's theory (ζ, II. 399, and III. 92).

[4] In the admirable study of the subject in his *Finanztheoretische Untersuchungen*, which I had not seen when writing before, in the Economic Journal, 1897, on this topic.

[5] In the symbols which we have employed above, the gain is of the order

I don't know that there remains anything worth fighting for under this head. I quite admit—I never denied—the efficacy of friction. Professor Seligman appears to admit the abstract theory when, in a passage which has been already quoted, he reasons: "the producer, who has advanced the tax, will increase his price only to that point where the smaller sales are compensated by the higher price, so that his net profits will still be at the maximum" (p. 204). If any difference of opinion remains, I surmise that it relates to the assumed continuity of the demand-curve (and other economic functions).[1] I have thought it legitimate to assume, not only with Professor Marshall, that "the demand for a thing is a continuous function," [2] but also that, like the continuous functions which we ordinarily meet with in nature, it is not continually changing its character in respect of convexity or concavity.* If the gross receipts curve represented by *Ss* on our diagram 2 is concave to the axis of $X$ at the point $S$ corresponding to maximum net profits, it may be assumed that, in general, in the great majority of cases which occur in ordinary practice, the curve will retain that character, as we move away from the point, for some finite distance. On this ground, it may be assumed as generally true that the imposition of a tax will tend to raise price.

In the postulate of continuity lies the answer to the difficulty raised by Professor Seligman in the following passage:—

"Cournot states that the tax must always be shifted (except in the cases mentioned in the preceding note.") [The cases quoted from the article in the ECONOMIC JOURNAL, 1897 (S, II. 91). Does Cournot make both these exceptions?] "Professor Edgeworth (ECONOMIC JOURNAL, VII. p. 405) thinks that this is true 'in general.' Later, when hard pressed by Professor Graziani, he seeks to maintain his position by assuming 'that the change of price is small,' 'by taking $\Delta p$ *sufficiently small*' (ECONOMIC JOURNAL, VIII. p. 235). But is it fair to assume that a small change of price is 'more general' than a great one? And would Professor Edgeworth's elaborate formulæ all hold good, if the change of price were substantial?" (p. 276).

Certainly, the formulæ hold good for substantial changes of

$\frac{1}{2}\tau\Delta x$; upon which, however, it may be remarked (1) that $\Delta x$, though in general of the same order, may be sometimes much larger than $\tau x$, (2) that $\tau\Delta x$ though small in relation to the *gross receipts* may be considerable in relation to the *net profits*.

[1] *Cp.* Professor Seligman *op. cit.* p. 278 (note): "The error of Professor Edgeworth seems to consist in the assumption that the demand curve is continuous."

[2] Preface to *Principles of Economics*.

* See below, ζ, II. 389.

price as long as the conditions of a maximum continue to be fulfilled, that is, presumably for some finite distance.* On the page following that which Professor Seligman has quoted, there is given an illustration, in which, as the tax is increased, the price will continue to rise up to the point where the monopolist's profits vanish. The rise of price attending increase of taxation may be interrupted when the demand curve (or some other function involved) changes its character in respect of concavity. Professor Seligman's own example affords a good instance. Taking the figures quoted above, with the omission of the tax, we have the subjoined data for the gross receipts curve. It will be seen

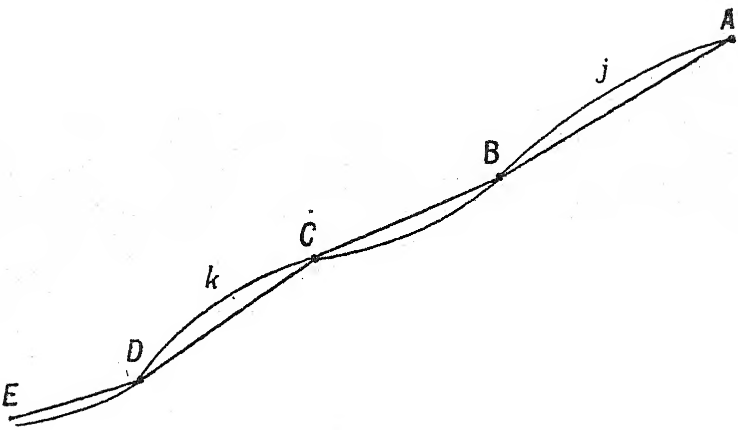


FIG. 3.

| | A | | B | | C | | D | | E |
|---|---|---|---|---|---|---|---|---|---|
| Output | 1000 | | 900 | | 825 | | 750 | | 700 |
| Price | 5 | | 5·25 | | 5·5 | | 5·75 | | 6 |
| Gross receipts | 5000 | | 4725 | | 4537·5 | | 4312·5 | | 4200 |
| Differences of ordinates | | 275 | | 187·5 | | 225·0 | | 112·5 | |
| Differences of co-ordinates | | 100 | | 75 | | 75 | | 50 | |
| Slope | | 2·75 | | 2·5 | | 3 | | 2·25 | |

that as the price is lowered from 6 labelled E to 5 labelled A, the output of course being concomitantly increased, the gross receipts continually increase. But the rate of this increase is not continuous. It is less rapid from *E* to *D* than from *D* to *C*, and from *C* to *B* than from *B* to *A*. The simplest continuous curve corresponding to the data would presumably be of the form indicated by the sinuous line in the annexed diagram. The reader will be so good as to substitute in imagination a curve of this kind, instead of the gross-receipts curve *Ss*, in our diagram 2, on a former page. By the reasoning there given, or referred to, it

* *Cp.* below, p. 167.

appears that, with the imposition, and gradual increase from zero, of a specific tax, the output will decrease, and the price will increase, as long as the curve is concave, say up to the point $j$. At that point the sort of index which we may conceive moving along the curve, stops. It may stop some time at $j$; or it may almost immediately fly over to the next crest, between $D$ and $C$. It cannot descend to $C$ (the law of cost being supposed constant, $2 per unit) unless the curve is very unusually complicated. It will continue to move on to the point $k$, where the curve again changes its curvature, after which another jump may sooner or later ensue.

Because the mathematical investigation advances by tentative steps it is not precluded from going in the direction of the rise of price as far as any other method, provided that the conditions of a maximum are secured. Without that condition, the calculus is helpless: it "fears to tread" where the ground is insecure; contrasted in that respect with other methods, but not to its disadvantage.

(5) Professor Seligman draws out his arrays of figures for another pitched battle on the question whether "a tax on monopoly gross receipts *must* raise prices"; maintaining that, "although it is generally true that a tax on monopoly gross receipts will raise prices, this conclusion not necessarily follows." From the mathematical point of view the distinction between this case and the preceding is unimportant, with respect to the purpose in hand; as appears from Cournot's analysis.[1] Suppose with Professor Seligman that a tax of 10 per cent. is imposed on gross receipts, then the amount which the monopolist seeks to maximise is $\frac{9}{10}$ gross receipts — total cost; or, $\frac{9}{10}$ (gross receipts — $\frac{10}{9}$ total cost). Accordingly, the change of price consequent on the tax will be the same as if, instead of the tax *ad valorem*, there had been an increase of the total cost by $11{\cdot}\dot{1}$ per cent. The effect of such an increase of cost on price is identical with that of a specific tax in the case of constant returns, and of the same general character in the case of varying cost: as may be seen from our diagram 2, by observing that any point on the cost curve is pushed upwards as before, not now to the extent of a certain proportion of the abscissa, but to the extent of $\frac{1}{9}$th of the ordinate. The same theoretical necessity, the same practical reservations, apply to the fifth as to the fourth issue.

Let us pause after these five rounds. I have noticed some half-a-dozen other instances in which my distinguished opponent's

[1] *Principes Mathématiques*, Art. 41, ch. vi.

conclusions are diametrically opposed to those which are deducible by mathematical reasoning. But I think it best to confine the present discussion to issues which have been already raised; respecting which a difference of conclusion may fairly be ascribed to the difference of method rather than a mere slip on either side.

II. I go on therefore to consider some general reflections on the mathematical method which the author has prefaced to the discussion of particular theorems.[1]

Here is one of these reflections.

(1) "The mathematical study of the pure theory often assumes a simplicity of condition which does not actually exist; it purposely neglects the all-important element of friction and constructs hypotheses irrespective of their agreement with the facts of actual life" (p. 173).

I quite admit that mathematical reasoning—like all abstract reasoning—has its abuses, as well as its uses. I only enter a *caveat* against its being supposed that this remark is particularly relevant to the present discussion. The abstract questions which are at issue are understood in the same sense by both parties; there is no reason to suppose any difference of opinion as to the value of the right conclusions. What the impartial spectator has to consider is whether the party that dispenses with mathematical reasoning obtains the true answers. To cry out *Ne sutor super crepidam* does not prove that it is possible to make good shoes without the proper tools.

Again Professor Seligman remarks:

(2) "The chief advantage of the mathematical method is seen in the use of diagrams where an intricate point which involves the simultaneous consideration of several causes can be illustrated with greater brevity and clearness than in any other way. But when we proceed from diagrams to the higher algebra, the use of the mathematical method sometimes leads to refined calculations of more importance to the mathematician than to the economist, and of little perceptible use in solving any practical economic problems" (p. 173).

This unqualified preference of diagram to symbol appears to me to be exaggerated. When the causes to be simultaneously considered are numerous, diagrams are apt to become helpless. Thus in order to treat our first question diagrammatically, it would have been necessary to resort to *solid* geometry. And, as there are only three dimensions of space, even solid geometry

[1] *Op cit.* pp. 172–3.

would be inadequate to illustrate the problem of *three* classes of fares. Even with respect to the other issues, where we are concerned with only one commodity, the use of symbols appears to me to have some advantage. I propose to illustrate this statement by restating in symbolic language, specially addressed to the mathematical reader, solutions of the four problems—(2) to (5) inclusive—which have already been treated otherwise.

Let us begin with Cournot,[1] by considering an indefinitely small tax or addition to taxation, $i$ in his notation, or, as we might say, $\Delta\tau$. It follows from Cournot's reasoning that the increment of the price consequent on an increment of taxation may be expressed in terms of the following quantities: (1) the price, say $p$; (2) the rate at which the total cost increases with the product, say $c$; (3) the rate at which the increase of the total cost attending an increment of product increases with the increase of product, say $c'$; (4) the "elasticity," * or rate at which the amount demanded diminishes as the price is increased, say $e$; and (5) the rate at which the "elasticity" increases with the increase of price, say $e'$. Substituting these symbols in Cournot's expression for the change of price consequent on a tax (in his Art. 38, or rather in the expression which he gives for the change of price consequent on an increase in the cost of production equivalent to a specific tax in his equation (4), Art. 31) we have, *mutatis mutandis* [2]

$$\{-2e - c'e^2 - e(p-c)\}\Delta p = -e\Delta\tau.$$

Whence $$\frac{\Delta p}{\Delta\tau} = \frac{e}{2e + c'e^2 + e'(p-c)}.$$

To apply now this formula to the problems in hand. We see at once that to a (positive) increment of taxation corresponds an increase of price. This proposition holds good alike for specific and *ad valorem* taxes—our (4) and (5) (compare Cournot, Art. 41). And what is true of an indefinitely small increase of taxation, is true of a finite increase, so long as the denominator in the expression for $\frac{\Delta p}{\Delta\tau}$ continues positive; that is, I think we may say "in general"—in the long run of cases—to some finite distance from the point at which we started, as shown by Cournot, Art. 32.

[1] *Principes Mathématiques*, ch. vi. Art. 38.

* As announced above (p. 157), inverted commas are applied to elasticity in the popular sense corresponding to Cournot's $F'(p)$, when it becomes important to distinguish that sense from the strict definition corresponding to Cournot's $F'(p)p/F(p)$. The distinction becomes significant when the *second* differential coefficient makes its appearance (see $\zeta$, p. 394).

[2] It will be observed that his $F'(p)$ is identical with our $-e$, his $\psi'(p)$ with our $-c\,e$, his $\phi'(D)$ with our $c$, his $\phi''(D)$ with our $c'$, his $F''(p)$ with our $-e'$.

As he says, "this method of demonstration should be borne in mind, as it will be frequently recurred to." Bearing it in mind with respect to the remaining problems above designated, (2) and (3), we need only examine how $\frac{\Delta p}{\Delta \tau}$ is affected by the incidents in question, namely, variations in the law of cost, and variations in the "elasticity."

Considering the formula above given, we see that both the numerator and the denominator [1] of the expression for $\frac{\Delta p}{\Delta \tau}$ being essentially positive, $\frac{\Delta p}{\Delta \tau}$ must decrease with the increase (and increase with the decrease) of $c'$, other things being the same. The only other relevant things are $e$, $e'$, $p$ and $c$. And the only significant question is whether we have any reason to think that any of these quantities is likely to be greater or smaller when $c'$ is greater, in general in the long run of cases. I submit we have no ground for thinking that there is any *correlation* between $c'$ and any of those variables. Accordingly in the long run the rise of price consequent on any assigned increase of taxation is likely to be greater the smaller $c'$ is. A particular case of this proposition is that the rise of price is likely to be greater when $c'$ is negative than when $c'$ is positive: in other words, higher when the law of increasing, than when the law of diminishing, returns prevails. That is, understanding those laws as I have defined them. There is no direct connexion between increasing and diminishing returns in the other sense. But the proposition which has been enunciated is true also in the second sense so far as the attribute, which forms the first definition, is apt to be attended with the attribute which forms the second definition [2]—that is, possibly, very far.

We come lastly to problem (3). How is $\frac{\Delta p}{\Delta \tau}$ affected by the increase or decrease of $e$? It is quite a relief, after the monotony of contradiction, to have to admit that I have committed a slip at this point. In my former version of the theory [3] in the expression for "the increase of price due to a small tax" corresponding to the expression for $\Delta p$ just now written, I put, for the sake of simplicity, a single symbol, $B$, for what I now call $-c'e^2$. And that was all right for the immediate purpose in hand. But in

[1] The negative of this denominator being, as pointed out by Cournot, Art 31, "necessarily negative, according to the well-known theory of maxima and minima."

[2] *Cp.* above, p. 70.

[3] ECONOMIC JOURNAL, Vol. VII., p. 227, note 2. [Omitted from S.]

applying the formula to enunciate the effect of a change in $e$, I treated $B$ as constant,[1] forgetting that it involved $e$.

The following is, I now think, a more correct statement. In any given case it is impossible to say whether the increase of elasticity conduces to the increase or the decrease of the efficacy of a tax to raise price; unless we are given not only $c'$ (which may be supposed), but also $e'$, involving the *curvature* of the demand curve, which is not, I think, usually given,[2] even as to sign, much less with the quantitative precision which would often be necessary for the present purpose.

But the expression for $\frac{\Delta p}{\Delta \tau}$, though perfectly indeterminate for any particular case, may afford, I think, a certain presumption for the long run. Suppose the sign of $c'$ to be given, *e.g.*+, the law of decreasing returns prevailing. Then in the long run of cases—while $e'$ is now positive, now negative in sign, now large, now small in absolute quantity—for a majority of those cases an increase of $e$ would be attended with an increase in the denominator of the above-written expression for $\frac{\Delta p}{\Delta \tau}$, or rather what it becomes when both numerator and denominator are divided by $e$, and, therefore, a decrease of the ratio under consideration. Conversely, when the law of increasing returns prevails, $c'$ being negative, an increase of elasticity is likely to be attended with an increase in the efficacy of taxation to raise price.

To the extent of the former clause I have to retract my original statement. But I am still able to affirm universally, without reference to the law of cost, the contradictory of Professor Seligman's theory that "the greater the elasticity of demand the more favourable—other things being equal—will be the position of the consumer"; if the situation of the consumer is tested, as it ought to be, not so much by the rise of price as by the loss of *consumers' surplus*. Employing the proper criterion of the consumers' welfare, we may affirm, "the greater the elasticity of demand the more *un*favourable—other things being equal—will be the situation of the consumer."[3]

[1] ECONOMIC JOURNAL, Vol. VII., p. 228, note 4 continued from p. 227.

[2] As to this unknown element, see E, I., p. 135, and $\zeta$, II. 394.

[3] The *loss* of *consumers' surplus* consequent on raising the price from $p$ to $p + \Delta p$ is approximately (*cp.* E, pp. 117, 132) $x\Delta p$; where $x$ is the product, which, by Cournot's equation (3) of ch. v. $= e(p-c)$. Therefore the loss of *consumers' surplus*

$$= e(p-c)\Delta p = \frac{(p-c)e^2\Delta\tau}{2e+c'e^2+e'(p-c)} = \frac{(p-c)\Delta\tau}{\frac{2}{e}+c'+e'\frac{(p-c)}{e^2}}.$$

This expression for the *loss* of consumers' surplus is always *positive*, both the

For the purpose of obtaining propositions in Probabilities, as the preceding may be described, I submit that symbols seem to have an advantage even over diagrams—not to speak of numerical illustrations. Thus it may be objected to our diagrammatic proof [1] of proposition (2) that some *a priori* knowledge derived from analysis is required to guarantee the legitimacy of our supposition that the law of cost only is varied, other things being preserved constant. A diagrammatic proof of proposition (3) would be even more precarious.

(3) One more of Professor Seligman's general reflections :—

"It may even be doubted whether the mathematical method has independently discovered any important principle susceptible of practical application that could not have been also expressed in everyday language."

Those who have followed the preceding discussion may be disposed to admit that, if the mathematical method does not itself discover important practical principles, it may at least be usefully employed to test the principles which a distinguished practical economist regards as important. If it is worth his while to employ some pages of economic analysis and numerical examples in endeavouring to prove those principles, it is worth our while to employ some lines of symbol in endeavouring to disprove them.* The negations are often also affirmations, but not very confident ones. It is as if an opponent should prophesy that the last week of April or May would be the coldest part of the month. The reply is that what we know about the matter points in a contrary direction: there is a constant cause making for greater heat—namely, the position of the earth relatively to the sun—in the latter part of each month; though doubtless that tendency may be counteracted by unpredictable vicissitudes of weather. What if the more abstract part of political economy, like the more sublime part of astronomy—that which contemplates the mechanism

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numerator ($= \Delta\tau x \div e$) and the denominator (for the reason given in note 1 to p. 168) being positive. The loss is in the long run greater the greater $e$ is, since one term of the denominator $\frac{2}{e}$ becomes less as $e$ becomes greater; while the other term of the denominator which involves $e$ may be treated as inoperative (if not diminishing), on an average, in the long run of all possible values (positive and negative) of $e'$, in our ignorance of $e'$. Thus the loss of consumers' surplus is likely to be greater the greater the elasticity.

[1] Above, p. 155.

* There are here omitted some sentences referring to the *second* edition of *Shifting and Incidence*, but less pertinent to the *third* edition.

of the heavenly bodies external to our system—were not at present susceptible of direct practical application, the mathematical theory of economics might still confer a benefit analogous to that which the mathematical theory of astronomy conferred when it discredited the pernicious pretensions of the astrologers. There are those who think that even of the received economic analysis the most important function is *negative*. Thus Mr. Leslie Stephen :—

"Political economy, as I venture to think, has been especially valuable in what I have called its negative aspect. It has been more efficient in dispersing sophistries than in constructing permanent theories. Economic writers have exploded many absurd systems. They have so far cleared the way for an application of sounder methods. But the complexity of the problem is so great. . . ." [1]

The sort of sophistry which has been eradicated from the general field of economics by the received *organon* finds a still virgin soil in the nooks and corners of which the cultivation requires the implements of mathematics.

The trenchancy of this criticism is not inconsistent with the diffidence which is proper to an inexact science, and the respect which is due to a high authority. For on the one hand, the region of hypothetically abstract theory, to which this polemic is confined, forms the one territory of economics in which issues may be fought out without compromise, there being a right diametrically opposed to the wrong. And on the other hand, it is no discredit to the ablest combatant, when he is unprovided with the proper weapons, to succumb.

[1] *Life of Fawcett*, p. 149.

## (G)

## RAILWAY RATES

[THIS article, which was published in the ECONOMIC JOURNAL, 1912, as the second section of the series announced as "Contributions to the Theory of Railway Rates," deals with some leading problems in Railway Economics. The function of discrimination is investigated on the lines of Dupuit. There comes up for reconsideration the vexed question of the relation between through fares lowered by competition and fares to intermediate localities which are subject to monopoly. The discrepancy in this matter between expert practice and abstract theory is explained by the circumstance that the theory may be too abstract, not taking into account the monopolist's concern for interests in the distant future. The monopoly also may be not pure, but mixed with competition or altruistic motives. A return to the subject is promised; but this is one of the promises which were swallowed up by the War].

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The classical economists rather anathematised than analysed monopoly.[1] It was reserved for Cournot to cultivate this neglected branch of economics; gathering the first-fruits of the mathematical method. Cournot and his mathematical successors have fully discussed what may be called the leading case: that of a monopolist dealing with a whole class of mutually competitive customers at one and the same price. Some acquaintance with the laws governing this comparatively simple case is here presumed. My contributions are directed to a subject less generally studied,[2] the case in which the monopolist *discriminates* between different classes of customers. I build upon the foundations laid by Dupuit.[2]

[1] For instance, Adam Smith's dictum, "The price of monopoly is upon every occasion the highest which can be got" (*Wealth of Nations*, Book I. chap. vii.), J. S. Mill's dictum, "Monopoly value does not depend on any peculiar principle, but is a mere variety of the ordinary case of demand and supply" (*Political Economy*, Book III. chap. ii. § 5), seem wanting in precision.

[2] See his epoch-making papers, *De la mesure de l'utilité des travaux publics* and *De l'influence des péages sur l'utilité* in the Annales des Ponts et Chaussées, 1844 and 1849, which will be found among the periodical publications in the library of the British Museum. Some extracts from Dupuit's papers are given in my article in the ECONOMIC JOURNAL, September, 1910.

The corner-stone of this building is formed by a conception which Dupuit introduced under the designation "rente des acheteurs": the money-measure of the benefit accruing to purchasers from obtaining articles which they purchase at a certain price, while they would have been willing to give more for those articles rather than go without them altogether. The sum of money designated by the term in question may, I think, be an object of science as well as the sum designated by the more familiar term *price*. The monetary equivalent of total utility may be as objective as the monetary equivalent of final utility. It should be observed, too, that often the *Rente des Acheteurs* with which we are here concerned does not consist only of *Consumers' Surplus* in the phrase adopted by the second founder of the theory, but also of a certain *Producers' Surplus* which consists of money, and does not require, like Consumers' Surplus for the most part, to be evaluated by an unusual or hypothetical transaction. Thus, if a railway lowers the rate for carriage of coal to a residential and manufacturing town (dependent on that railway for its supply of coal), not only will there be a gain of Consumers' Surplus to those who use coal for domestic purposes, but also the manufacturers becoming able to extend their use of coal in various directions will presumably secure a greater surplus of money profit. I propose to subsume the two kinds of advantage which may accrue to the purchasers of monopolised services under the title "Customers' Benefit."

The theory which I attempt to construct is based mainly on the first principle of pure economics, the prevalence of self-interest. In the words of Professor Cohn, comparing the different motives by which railway managers are actuated, "by far the weightiest are assuredly the egoistic motives."[1] In the words of another high authority,[2] "the constant effort of every railway company [is] to secure the volume of traffic and to maintain the fares that will jointly yield maximum net profit." "The main purpose of the railway manager is to secure present or prospective profit for the stockholders."[3] But while reasoning from this premiss, I do not forget that a concrete railway company is far from being a perfect monopoly; and I will point

[1] "Weitaus die wichtigsten sind allerdings die egoistischen." *Englische Eisenbahnen*, vol. ii. p. 398, and context adducing evidence that "das Eigennutz" is the predominant motive.

[2] Johnson and Huebner, *Railway Traffic and Rates*, p. 227. The words relate to passenger traffic, but may safely be generalised. Compare the context, p. 216. A qualification of this assumption, made by the distinguished writers, will be noticed in the sequel.

[3] *Op. cit.*, p. 228.

out how the deductions from the abstract principle require to be modified by other considerations.

(1) *Discrimination due to differences in demand.*—The most characteristic case of discrimination resulting from monopoly occurs when different charges are made for like commodities solely on account of differences in value to the purchaser, and quite irrespectively of differences in cost to the producer. Examples of this case too familiar to require citation occur throughout the wide fields of railway practice [1] designated by the terms Classification and Local Discrimination.

To explore the consequences to the customers of such discrimination, let us at first suppose that no very great difference of rates—no "wrench in commercial conditions" in the phrase of an experienced railway manager [2]—is caused by the introduction of the discrimination. Let us further provisionally assume that the company's interest in the two kinds of traffic—say through traffic and local traffic—is not very unequal. We have then the typical case which I have discussed at length in a former paper.[3] It is there shown that the perfectly self-interested monopolist tends to exploit the customers in such wise that their last state will be worse than their state prior to discrimination. But, as it is further shown—by an extension of a theorem pointed out by Dupuit and applied by M. Colson—the monopolist has not much interest in pushing his exploitation up to and beyond the limit at which discrimination begins to be detrimental to the customers as a whole. For a small consideration the monopolist can probably be induced to adopt a set of prices such that the customers as a whole—as well as the monopolist himself—may be gainers through discrimination.[4] If he does not insist on extracting the uttermost farthing, discrimination will result in an increase of Customers' Benefit.[5]

[1] As shown by all intelligent writers on the economics of transportation, with particular lucidity by Hadley and Acworth.

[2] E. P. Ripley in the context of a passage quoted in the sequel.

[3] *Applications of Probabilities to Economics*, ζ, II. 407.

[4] The instances of discrimination alleged by Schipfer in his interesting study on the passenger service of Prussian railways (*Volkwirtschaftliche Studien*, Berlin; Heft 209) as detrimental to the public—for instance, between "return" and ordinary fares—are hardly relevant here, so far as the allegation rests on the difference in the value of money to different classes of the community. This is a consideration not taken into account here.

[5] This last proposition is a new corollary, which the mathematical reader will have no difficulty in deducing from the theory given in the paper of 1910; upon the assumption not only that the coefficients $\alpha$, $\beta$, $\gamma$, etc., pertaining to the demand curves (*loc. cit.*, p. 459, note), are small, but also that the monopolist takes no account of amounts (of profit) less than what correspond to the squares of those

As we leave the limiting case in which the receipts prior to discrimination are nearly equal and the discrepancy in prices produced by discrimination is inconsiderable, the probability in favour of the conclusions enounced becomes weaker.[1] The lamp of Probability is dimmest when the inequality between the original demands is considerable, but not immense. Penetrating this obscure central region we emerge into daylight[2] as we approach a new limit characterised by the extreme inequality of the receipts prior to discrimination. One class has now so small a demand, in the absence of discrimination, that the alteration of its price does not sensibly affect the other class constituting the bulk of the customers (prior to discrimination). There are two varieties of this extreme case; the rate fixed for the exceptional class may be either raised or lowered. An example of the first variety would occur if millionaires were treated as *corvéable à discrétion,* in Mr. Acworth's phrase. But probably in the management of railway traffic this variety is much rarer than the converse one in which rates are lowered for outlying classes. This category includes the cases in which judicious managers "create traffic"[3]—"excursion business, handled at reduced fares," in contrast to "the regular business at standard fares,"[4] "extra traffic that will not move without special conditions."[5] Here we may place walking and cycling tickets, week-end, and "long-date week-end" tickets,[6] special terms offered to passengers on the occasions of exhibitions, football matches, etc.; with like discrimination in favour of producers. Now in all such cases there is a pure gain of Customers' Benefit; since by hypothesis the position of the bulk of the customers is unaltered, while

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small magnitudes. The corollary, it may be observed, is true independently of the form of the demand-function, since in the expansion of those functions the terms involving higher powers than the second may be neglected.

[1] *Loc. cit.*

[2] Theoretically the light does not return until we have crossed the new limit into an outlying region occupied by customers who have *no* demand at the original (undiscriminated) price. But practically it may be often assumed that the very small demand of a special class will not have a sensible effect on the rates fixed with regard to the demand of the majority.

[3] See the lively directions for creating traffic quoted by Johnson and Huebner, *Railway Traffic and Rates,* Vol. II. p. 197.

[4] *Loc. cit.,* p. 221. [5] *Loc. cit.,* p. 189.

[6] Many such instances are given by H. Marriott, *The Fixing of Rates and Fares,* chap. vi. Perhaps some of the instances are not quite at (or beyond) the limit under consideration. Thus, the demand for week-end journeys might well be so great and of such a nature that, if special week-end fares were to be abolished and a uniform fare adopted, that undiscriminated charge would be—practically, as well as theoretically—lower than the present standard fare.

the remainder are enabled to purchase a commodity of which the price was previously preventive.

(2) *Discrimination due to differences in cost.*—An equally simple and perhaps more familiar case arises when two classes of customers [1] between whom it is proposed to discriminate have the same demand for a commodity which it costs more to supply to one than to the other. For instance, the two classes may be the residents in two localities, whose dispositions with respect to local journeys are identical; but the operating expenses [2] may be greater—say on account of the steepness of the gradients—in one place than the other. The reader will rightly presume, though probably for the wrong reason,[3] that when the price of the two services is not constrained to be uniform, the price of the service which it costs more to supply will (theoretically) be higher. But preconceptions will be of little avail to answer the question whether under the circumstances the customers as a whole will be benefited by the discrimination. The Ricardian doctrinaire will presume that prices proportional to costs of production form the best possible arrangement. The undisciplined Socialist may presume that equal charges for classes whose wants are identical are productive of maximum satisfaction. But both these contrary presumptions are incorrect. The first thesis is only approximately true; the second is false.[4]

(3) *Discrimination due to differences in both demand and cost.*—Now let us compound the two preceding simple cases. For instance, the discrimination between first- and third-class passengers' fares (in England) is, I suppose, based partly on the different requirements of the passengers, and partly on difference in the cost of equipment. In this case the same line of cleavage separates the classes which are discriminated in respect of demand and in respect of cost.[5] More generally, it may be conceived that the two classes differing in one of those respects, for instance, the residents in the two localities for which the cost of carriage

[1] It may be well to repeat here the advertisement which I gave at the outset of the cognate paper in the ECONOMIC JOURNAL for 1910 :—" I shall for convenience of enunciation confine my statements to the variety in which only two species are discriminated; but the propositions thus enunciated are readily adapted to any finite number of species."

[2] The *general* expenses are not relevant to the present theory.

[3] On the strength of Ricardian premisses inappropriate to the present inquiry which relates to pure monopoly. The true reason is to be found in Cournot's theory concerning the effect of a rise in cost of production on the price of a monopolised commodity.

[4] For proof of these and following statements see the paper on the Application of Probabilities, Section VI., $\zeta$, below, p. 387, *et seq.*

[5] *Loc. cit.*

was different, have a uniform demand only on an average; each class capable of being broken up into species, such as travellers by fast and by slow trains. We may then employ first one of the above propositions, then the other, to obtain conclusions analogous to those which have been enounced.

It remains to notice the modifications of the simple propositions which arise when we introduce *correlation* [1] both of demand and cost. For instance, the demand on the part of passengers for first-class and that for third-class accommodation are *rival.* The demands for the carriage of the passengers themselves and for the carriage of their luggage (where, as in Prussia, a separate charge is made for luggage) are presumably *complementary.* In the case of special tickets for "parties" [2] we have an instance of *complementary demand*, so far as the desire of each for the trip is heightened by the pleasure of company; and of *joint cost* so far as the largeness of the party enables the economies of production on a large scale to be realised.[3] But it will not be necessary to examine in detail the variety of cases which are constituted by these complications. The subject is particularly open to the remark which Mill quotes from Montesquieu: "Il ne faut pas tellement épuiser une chose qu'on ne laisse rien à faire au lecteur." The reader is invited to ascertain for himself, with the aid of the notes in the former paper,[4] that the propositions above proved for the simpler cases of demand and supply may be extended to cases complicated by Joint Demand and Cost; the probability becoming more *a priori* and of less practical service as the complications increase. The mathematical reader will have no difficulty in making these generalisations if he bears in mind the essential characteristics of our theory, namely, (1) that monopoly profit is at a maximum [5] (before and after discrimination), and accordingly that the data must be such as to fulfil the usual criteria of a maximum; (2) that the coefficients which determine the extent of the discrimination are, at least, initially small; [6]

[1] The term is defined and divided, S, II. 72, 73.

[2] For instances of such arrangements, *see* the passages of Johnson and Huebner's *Railway Traffic and Rates*, referred to above, p. 175.

[3] See, as to the relation of Joint Cost and Increasing Returns, passages referred to in the Index.

[4] See $\zeta$, p. 423, *et seq.* The treatment of Correlated Cost is exemplified below, p. 184, *note.* *Cp.* S, II.

[5] In accordance with the fundamental premiss postulated above, p. 173.

[6] Relatively small (such as the coefficients $\alpha$, $\beta$, etc., employed in the note to p. 174, above); partly in accordance with the practice of railway managers (alluded to below, p. 190), but principally in virtue of a method of reasoning introduced by Cournot and largely applicable in mathematical economics; as to which see Index s.v. *Differentials and Finite Differences.*

(3) that the usual postulates of *a priori* probabilities [1] are granted; in particular, it is assumed that if two quantities $P$ and $Q$ take on from time to time different values (ranging through tracts of the same order of magnitude), such that $P$ is always positive, $Q$ as often positive as negative, then (under the circumstances characterising the class of problems with which we are dealing) the sum $P + Q$ is probably positive.

With reference to the complex cases, it may be well perhaps to explain what is implied in my use of the evasive term Joint Cost. When it is said in the present context that two discriminated commodities have a joint cost, it is meant that when the production of one is increased by an amount such as that which results from the discrimination contemplated,[2] then the increased production of the other commodity becomes less costly. It is true that most of our examples seem to imply Joint Cost on a different scale. Thus, the two parts of the same railway which were instanced in our second subsection would, no doubt, have a joint cost in virtue of the initial expenses of the railway. But we might equally have supposed two distinct railways, owned by the same company, similar as to the requisites of customers, differing only in that the cost of haulage is greater on one of them than in the other. So with respect to our first problem we might suppose two separate lines (owned by the same company) on one of which lime destined for agricultural purposes, on the other lime destined for architectural purposes is hauled, at different rates, though the cost (per ton) of haulage is the same.

It thus appears that the distinction drawn by Professor Taussig [3] between the discrimination resulting from Joint Cost

[1] [As to *a priori* Probabilities, see passages referred to in Index.] In the case specified in the text it is assumed that the frequency with which $P$ and $Q$ assume different values in the long run (between certain limits) is approximately uniform, in accordance with common experience as to the behaviour of statistical quantities (the sort of assumption approved by Karl Pearson, *Grammar of Science*, p. 146, 2nd edition).

The argument in the text may be illustrated by the following transaction:—Two digits are taken from mathematical tables at random, or from the expansion of such a constant as $\pi$. I (1) give you a number of shillings equal to the *first* digit, and (2), according as the *first* digit is odd or even, either I give you or you give me a number of shillings equal to the *second* digit. In the long run, formed by a series of such trials, you would stand to gain. The proposition remains true when the ranges of the two elements are different; for instance, one of the two component digits being excluded from the values 8 and 9. But the proposition is of course less useful when the order of the constantly positive element is small compared with the element of inconstant sign.

[2] As to the propriety of specifying the magnitude of the "dose" considered with reference to Joint Cost, and generally Laws of Return, see **C**, above, p. 65, *et passim*.

[3] *Quarterly Journal of Economics* (1891); reprinted in Ripley's *Railway Problems* (p. 140, *et seq.*).

and that which results from Monopoly, however important in general, is not particularly relevant to the benefit accruing to customers which we have in view. This sort of benefit may be obtained from discrimination without Joint Cost in a regime of Monopoly, and with Joint Cost in a regime either of Monopoly or of Competition. Our withers are unwrung by the observation that " people constantly confuse the principle of joint cost with monopoly. To charge what the traffic will bear under the former principle is for the public interest, to charge what it will bear under the latter principle is against the public interest." [1] The public interest which most writers outside the school of the *Ponts et Chaussées* connect with joint cost and discrimination of price is the circumstance that but for such discrimination the production is apt to be unprofitable and therefore impossible.[2] But the public interest which I here, after Dupuit, emphasise, is one quite distinct from that familiar advantage. It is sometimes, indeed, superadded thereto, but it often exists independently. It consists in minimising through discrimination that loss (*perte sèche* in M. Colson's phrase) of customers' benefit which is apt to result from unitary price. Doubtless competitive joint-cost is more in the public interest than monopolistic joint-cost, other things being the same. But other things are likely not to be the same, since monopoly is more favourable to discrimination.[3]

(4) *Changes in demand.*—Having now considered the influence of demand and cost as determining discrimination, let us go on to consider changes in those factors.[4] The question arises how will a change in the demand for one of the commodities discriminated affect the result of discrimination. The change may be supposed, with sufficient generality, to occur *after* the introduction of discrimination. The commodities which we have hitherto considered may be likened to two horses of different mettle at first yoked together and constrained to go at one and the same rate; afterwards unyoked and free to go each at its own pace. The question now arises, if, after the separation, one of the steeds be either spurred or reined, what will be the effect on

[1] Taussig, *Principles of Economics*, Vol. II. p. 495. *Cp.* our subsection 7, below.

[2] As in the " oyster-case " which Principal Hadley has made classic; referred to above, **C**, p. 93.

[3] As to the possibility of monopoly being better for the customer than competition, see **D**, p. 101, *et seq.*, and ζ, p. 406 *seq.*

[4] The relation between the following two subsections and the preceding three may be illustrated by the relation between § 2 and § 5 of Mill's chapter on *International Values,* dealing respectively with the level of values resulting from the opening of trade, and the change in the level resulting from a variation of the data.

the rate of the other? The metaphor suggests the true answer, namely, *nil*, in the absence of special relations, whether of rivalry or sympathy. Thus, if a Company has been free to adopt the rates that are most profitable at different distances (say, arbitrarily tapering rates), then should a fall in the demand for through traffic occur, through the construction of a competitive line passing through a distant point, that circumstance *per se* will not theoretically affect the fares for shorter distances. They had already been fixed at the amounts supposed to be most profitable; to alter them on account of some loss occurring elsewhere would mean but an additional loss of profit.

This deduction from abstract theory is at variance with the judgment of experts.

Thus, the experienced Albert Fink testifies that a hard-and-fast tariff (preventing discrimination) "would have the effect that they would have to increase their charges upon such portions of the road as they could control themselves." (Report of the Hepburn Commission, Vol. I. p. 68.)

So in the classical pages of C. F. Adams we read :—

"At one point" rates became almost literally nominal; residents "at other points would be charged every penny that they could be made to pay without being drawn off the railroad and back to the highway." (*Railroads*, 1878, p. 123.)

The high authority of Professor W. Z. Ripley may be quoted in favour of the prevalent doctrine :—

"In the constant pressure for reduced rates in order to widen markets, it is not unnatural that the intermediate points, less competitive probably, should be made to contribute an undue share to the fixed sum of joint costs." (*Political Science Quarterly*, Vol. XX.; 1906; *Railway Problems*, p. 489.)

And, again, Professor Ripley speaks of "the danger of local rates . . . being actually enhanced, or at least prevented from reduction because of an unduly low level of competitive rates at more distant points." (*Quarterly Journal of Economics*, 1909, p. 481.)

To the same effect the acute H. Turner Newcomb :—

"To these stations the relation of the railway is that of a monopoly, and from them the latter can and will . . . recoup all losses that may be sustained." (*Railway Economics*, p. 47.)

"Competition at terminal points impels most railways to charge relatively high rates at intermediate points, the traffic of which cannot be directed to other lines." (*Op. cit.*)

How strongly this doctrine of recoupment recommends itself

to enlightened common-sense appears from its continual recurrence in the reports and decisions of the Inter-State Commerce Commission. For example :—

"The greater the departure from the direct line, the greater would commonly be the necessity for lower rates on through traffic, and the greater the liability to have the charges on the local traffic increased to make the carriage of through traffic possible." (Quoted from the Decisions of the Inter-State Commerce Commission, Vol. I., by Hugo Meyer, *Regulation of Railway Rates*, p. 355.)

Through rates "must not be so low as to burden other business with part of the cost." (*Inter-State* Commerce Commission, Third Annual Report, p. 126.)

"If the rate is too low upon one article, in the end other articles pay too high a rate." (Quoted by Ripley, *Railway Problems*, p. 466.)

And who pays for this loss [occasioned by a roundabout service to competitive points]? "Ultimately the intermediate points." (*I.C.C. Annual Report*, Vol. XI., p. 45.)

A similar contradiction between abstract theory and expert judgment may be noticed with respect to the practice of giving free passes to passengers. Theoretically, if from a homogeneous [1] body of customers some are selected to pay for their journeys by some service to the general interests of the company, that variation of the terms for certain passengers does not *primâ facie* affect the rate for the other passengers; what was before the rate affording maximum profit still fulfils that condition.[2] But this theory is not in accordance with common opinion. Thus, the Cullom Commission complain (*inter alia*) :—

[1] If, as it is sometimes objected, the favoured persons belong to the wealthier classes (and so have a higher effective demand for passenger service)—"men of wealth and prominence who rode at the expense of others less able to pay" (*I.C.C. Annual Report*, Vol. III. p. 11)—then the discrimination would tend to *lower* the fares of the other passengers (by the theory of our subsection 1).

To continue our equine metaphor, we have (1) in the case of homogeneous demand a horse of like mettle with others running abreast of them at the same pace without any constraining yoke. The removal of such a one to be employed elsewhere does not tend to alter the general pace. But (2) if the steed removed is one of higher mettle, which ran at the same rate as others only through constraint, the removal of such a one tends to *lower* the general rate.

[2] One of the few writers whom I have found on my side in this matter is Marshall Kirkman, who enounces what I consider the (provisionally) true doctrine in his *Basis of Railway Rates*, p. 38 :—"Making of a low rate never has the effect to raise another rate. Each is independent." But I do not claim the alliance of a writer who holds that the charges "for railway service are governed by the same laws that fix the prices of other necessaries"—"fish or flour" (p. 26), "what we may term God's natural laws."

"That the cost of the passenger service is largely increased by this abuse" [the granting of passes]. (P. 801, quoted by the Elkins Commission.)

So A. B. Stickney :—

"To charge one person two prices for the sake of carrying another free" [seemed outrageous]. (*Railway Problems*, ch. viii.)

So the Inter-State Commerce Commission :—

"Favoured persons have been furnished free transportation at the expense of the general public by higher general charges to reimburse for gratuitous carriage." (Annual Report, III. p. 12.)

These antinomies between abstract reasoning and common opinion appear to be due principally, but perhaps not altogether, to the admitted inadequacy of the premiss stated at the outset, and so far employed without correction, the prevalence of perfect and perfectly self-interested monopoly.[1]

It remains under this head to consider the special cases in which there is a *correlation,* a sympathy, or rivalry,[2] either in respect of production or demand, between the commodities for one of which the demand is changed. Thus, a fall in the demand for through traffic (owing to competition at a distant point), not met by a lowering of through rates, might be followed by a shrinkage of the through traffic, with a loss of the economies attending production on a large scale that would involve a rise of the fares for shorter distances. To exemplify correlation of demand (between the commodities for one of which the demand is altered), suppose that in a country where a separate charge is made for passengers' luggage the demand for travel increases (without any decrease in the amount of luggage required on each journey); then the demand for the carriage of luggage would be increased, and therefore presumably the charge for carrying luggage might be raised.

The last conclusion requires some qualification, for it is one of the paradoxes of Monopoly as contrasted with Competition that a rise in demand, even under monopoly without discrimination, as shown in another paper,[3] is not necessarily attended with a rise in price. It is thus not exactly true to say with Ricardo that "commodities which are monopolised rise in price in proportion to the eagerness of the buyers to purchase them." [4]

[1] *Cp.* subsections 6, 7, and 8 below.

[2] The cases illustrated above, p. 177.

[3] Above, p. 144.

[4] *Principles*, ch. xxx.

This paradox has not any important, or rather not any *recherché*, analogy under the head of cost.[1]

(5) *Changes in cost.*—The antithesis between theory and common opinion recurs when we consider the case in which the cost of transportation is changed for one of the discriminated services. In the absence of correlations there is no reason why the rate for the other service should be altered. Thus, if Government should compel railways to carry particular classes—say soldiers or workmen—at unprofitable rates, yet, supposing that the railway was previously free to fix discriminating rates at its discretion, the other classes of customers need not suffer. Because the Government smites the Railway on one cheek, is that any reason why the Railway should smite itself on the other cheek by altering fares arranged to yield maximum profit? But such is not the received opinion, which is thus well expressed by Mr. Pratt:

"If workmen are to be regarded as a privileged class who must be carried to and from their occupations at fares or under conditions which do not pay a railway company, then it is obvious that the difference must be made up either by other classes of travellers, or by the general body of the traders." (*Railways and their Rates*, p. 41.)

So far supposing that there is no correlation between the discriminated services. Now let there be such a relation; and first, let there be a rivalry of demand as for first-class and third-class passenger service. In such a case, as I have shown at length in other papers,[2] a rise in the cost of one service may not only not cause a rise, but may cause a fall in the charge for the other service. The proposition is principally important as giving a shock to the obstinate convictions of the half-taught, who persist in transferring to a regime of pure monopoly the lessons which they learnt in their youth when competition held, in the books at least, undivided sway.

A similar interest attaches to a corresponding case of correlations between costs. For instance, suppose that goods and passenger services are so related, on a crowded line, that if the one is increased the other becomes more costly. Let the cost of the passenger service be increased by any cause not directly affecting the goods traffic. The Ricardian will rightly presume

[1] The analogue is the truism that an increase in the *total* cost of producing each amount of a commodity [Cournot's $\phi(x)$ as distinguished from his $\phi'(x)$] is not necessarily attended with a rise of price in a regime of pure monopoly.

[2] Above, E, p. 132; F, p. 149.

that the charge for passenger service will tend to rise. But he is likely to have a wrong opinion on the question whether the customers of the railway as a whole may be benefited by the change. By a parity of reasoning with that employed in the case of correlated demand, it may be shown that the increase in the cost of one service may quite possibly be attended with an increase of Customers' Benefit—more beneficial to the shippers and consumers of the goods than it is detrimental to the passengers. The proposition has some affinity to Dr. Marshall's celebrated paradox as to the conditions of maximum satisfaction.[1] Both theories conduce to the same purpose, to awaken old-fashioned dogmatists from their optimistic slumbers. But the analogy is not close, as appears from the observation that inelasticity of demand for the commodity of which the cost is raised is *not* a condition favourable to the effect here considered.[2]

[1] *Principles of Economics*, Book V. ch. xii.

[2] The general truth may conveniently be conveyed by way of a particular example. In a notation similar to that employed in an earlier paper (above, **E**, p. 132) let $p_1$, $p_2$ be the prices of the two commodities referred respectively to the prices at the point of equilibrium; let $x\,y$ be the corresponding, similarly measured, amounts of commodity. Let the laws of demand (not now correlated) be expressed by the equations:

$$p_1 = \tfrac{2}{3} + x - \tfrac{2}{3}x^2;\ p_2 = \tfrac{37}{24} - \tfrac{3}{4}y + \tfrac{5}{24}y^2.$$

Let the (now correlated) cost of producing the quantities $x$ and $y$, say $K$

$$= C + \tfrac{1}{3}x + \tfrac{1}{3}xy + \tfrac{1}{3}y$$

(where $C$ is a coefficient representing general expenses, which, with reference to the present operations, may be treated as constant). The values of $x$ and $y$ for which the profit of monopoly $(xp_1 + yp_2 - K)$ is a maximum are then as they ought to be, each *unity*.

Now let the (total) cost of production be increased by an expense proportioned to the amount produced of one commodity, say the additional expense $\tau x$, where $\tau$ is small. By the method explained in a former paper it will be found that the resulting increments of $x$ and $y$ are approximately,

$$\Delta x = -\tfrac{9}{14}\tau,$$
$$\Delta y = +\tfrac{8}{7}\tau.$$

The corresponding increment of Customers' Benefit is

$$-\Delta p_1(x + \tfrac{1}{2}\Delta x) - \Delta p_2(y + \tfrac{1}{2}\Delta y)$$
$$= -\Delta x\frac{dp_1}{dx}(x + \tfrac{1}{2}\Delta x) - \Delta y\frac{dp_2}{dy}(y + \tfrac{1}{2}\Delta y)$$
$$= -\Delta x(1 - \tfrac{4}{3})(1 + \tfrac{1}{2}\Delta x) + \Delta y(\tfrac{3}{4} - \tfrac{5}{12})(1 + \tfrac{1}{2}\Delta y)$$

Substituting for $\Delta x$ and $\Delta y$ the values above found and neglecting quantities of the second order, we have for the increment of Customers' Benefit $\cdot 5\tau$, that is, a *positive* quantity. [A smaller (positive) value was before inaccurately given.]

More generally put for the increment of Customers' Benefit (approximately)

$$-(x\Delta p_1 + y\Delta p_2);$$

where $\Delta p_1 = \frac{dp_1}{dx}\Delta x$, and $\Delta p_2 = \frac{dp_2}{dy}\Delta y$ (there being no correlation of demand). By reasoning of parity with that before employed (**E**, p. 131)

$$\Delta x = \tau\frac{d^2V}{dy^2}\Big/D^2;\quad \Delta y = \tau\frac{d^2V}{dxdy}\Big/D^2;$$

where $V$ as before is the monopolist gain, $D^2$ is necessarily positive, $\frac{d^2V}{dxy} = -\frac{d^2K}{dxdy}$

(6) *Future interests.*—" So far we have supposed the owner of a monopoly to fix the price of his commodity with exclusive reference to the immediate net revenue which he can derive from it." I use Dr. Marshall's words [1] to mark the transition to cases in which the monopolist may alter his price " with a view to the future development of his business." The development of his own business by developing the business of his customers, at a sacrifice of present to future profit, is more readily practised by the monopolist than by the entrepreneur in a regime of competition, as the monopolist has an assurance that the fruits of his sacrifice will not be snatched by a competitor. With this view intelligent railway managers often fix rates lower than those which would afford maximum profit in the present. To encourage future traffic " a railroad operating in a new territory may for a time offer to carry freights at rates which barely cover expenses." [2] They may even " haul materials at a loss." [3] Coal, hay, grain, " are sometimes carried at less than the total expenses." [4] Freight is sometimes carried at a loss to get some other freight that will pay more.[5] The consilience between the interest of the monopolist and his customers, which was before seen to be approximate, seems now to be complete. There is presented an optimistic view of *dynamical* discrimination comparable to Bishop Butler's doctrine that rational self-love and universal benevolence are nearly coincident in this life and completely when account is taken of the future.

It is true, indeed, that in the words of an able writer, " traders' and railways' interests are in the long run coincident." [6] But the traders of whom this is true are supposed to continue customers of the railways during the long run.[7] But the railways prevent many traders from having a long run. In the words of the Hepburn Commission, " in a speculative attempt to increase

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where $K$ is the total cost of production. In order that the increment to Customers' Benefit may be positive, $-x\frac{dp_1}{dx}\frac{d^2V}{dy^2} - y\frac{dp_2}{dy}\frac{d^2V}{dxdy} > 0$.

Whence $\left(\text{since } \frac{dp_1}{dx} \text{ and } \frac{dp_2}{dy} \text{ are both negative}\right) \frac{d^2V}{dxdy}$ must be positive. That is, the production must be complementary (joint). *Both* prices cannot fall; since $\frac{dp_1}{dx}<0$, $\frac{dp_1}{dy}=0$, $\Delta x<0$.

[1] *Principles*, sixth ed., p. 486.

[2] Johnson and Huebner, *Railway Traffic and Rates*, Vol. II. p. 317.

[3] *Op. cit.*, p. 366.

[4] *Op. cit.*

[5] Hepburn Commission, p. 2894.

[6] ECONOMIC JOURNAL, Vol. XIX. p. 477.

[7] The inadequacy of this supposition is strikingly illustrated by Frank Norris's story *Octopus*. But the exploitation of Dyke and other customers of the grasping Railway involved an element of fraud not here contemplated—typical of common cheats rather than common carriers.

business they favour one shipper at the expense of another."[1] A witness admits to having "acted as a fostering mother" to one customer but not so to another—"a small concern."[2] "A railroad has the life and death of the manufacturer in his hands," as Professor Ripley has said;[3] and in the United States it has exercised that power unscrupulously, has ruined some in order to build up others. The American railway manager has fostered or frozen-out manufacturers, has brought on or kept back cities at his arbitrary discretion, like the ruthless agent of transportation in the nether world who, discriminating between passengers across the Styx,

> "Nunc hos, nunc accipit illos,
> Ast alios longe submotos arcet."[4]

It is tenable, indeed, that even in Monopoly, as certainly in Competition, *laissez faire* is less detrimental than at first sight appears. Thus, perhaps, Professor Hugo Meyer is right when he affirms that if railway managers had had a free hand to establish "basing points" in Australia, they would have brought about that very decentralisation which has been vainly aimed at by governmental regulation.[5] Perhaps he has rightly described some evils attending unrestrained discrimination as but "growing pains."

Postponing the difficult questions just suggested, I have to add here one dark trait to the picture of dynamical discrimination. The immunity from vicarious suffering which was claimed on statical grounds[6] is not equally tenable dynamically. Statically, it may be unthinkable that because through traffic has become less profitable, therefore the profit of local traffic should be diminished by an alteration of rates. But dynamically, if the loss of immediate profit has disturbed the balance of present and future advantage, it is quite conceivable that local customers who were before spared for future development should now be sacrificed to present exigencies.

(7) *Action of competition.*—Several corrections of our provisional conclusions are required by the incompleteness of our first principle—the prevalence of perfect and perfectly self-interested monopoly. Firstly, Monopoly is seldom perfect. In spite of agree-

[1] Report, p. 64. [2] *Loc. cit.*, p. 62.

[3] Report of the Industrial Commission, Vol. XIX.

[4] "Quo discrimine," on what principle of discrimination, was the natural inquiry of an intelligent visitor; and the answer left him pondering and pitying the victims of discrimination under their hard, not to say unjust, fate (sortem iniquam).—*Aeneid,* Book VI.

[5] *Regulation of Railway Rates,* p. 301. [6] Above, p. 180.

ments and consolidations,[1] railways are apt to compete for the carriage of goods and persons, whether by the offer of lower rates or higher accommodation. Then there is the so-called "competition of markets,"[2] when the customers of different railways compete against each other in one and the same market. Each railway in order to preserve and increase its custom must moderate its charges for carriage to the common market; just as an intelligent trade union will not demand a rise of wages so great as to make the competition of the employers in a foreign market impossible. I must leave it to railway experts to evaluate the extent to which these kinds of competition are effective. My analysis resembles Mr. Asquith's Coal Bill in not having any figures inserted. It is safe to say with Professor Johnson, "The railways are only partial monopolies."

As some kinds of competition prevent railways from exploiting their customers, so another kind of competition tends to prevent railways from being exploited by Governments or customers acting in combination.[3] This is the competition between different industries for the funds of investors. In virtue of this competition the profits of railways (and like industries [4]) tend to be on a level with all the industries that are run by Companies. This is certainly an appropriate conception; but to what extent it excludes the conception of monopoly I do not feel competent to determine. Professor Taussig has assumed, provisionally at least, and for the sake of argument, that "a railroad's business is carried on under the circumstances of free competition."[5] Mr. Harry Turner Newcomb may be mentioned as having expressed the conception with peculiar clearness.[6] It must be admitted that a railway company is analogous to, or rather identical with, a capitalist seeking the most profitable investment. But it is to be remembered that the equation of profits which is deduced from this Ricardian principle is true only over "long periods." But there is reason to think that in concerns of such magnitude as railways the relatively "short period" is absolutely long. It is

[1] The degree of unification prevailing between American railways is well shown in the fifth chapter of Emory Johnson's *American Railway Transportation*.

[2] Johnson, *op. cit.*, p. 65. W. Z. Ripley, "Local Discrimination," *Quarterly Journal of Economics*, Vol. XXIII. p. 489 *et passim*.

[3] There is latterly much complaint that manufacturers, combined in the form of "trusts," impose hard terms on carriers.

[4] It is hoped that the reader of these pages will throughout retain in his memory what was stated at the outset (ECONOMIC JOURNAL, Vol. XXI. p. 346) that "railways" are here used as typical of the larger class of industries which have been described as "public works."

[5] *Quarterly Journal of Economics*, 1891, reprinted in Ripley's *Railway Problems* (p. 146).

[6] *Railway Economics*, pp. 75, 78 *et passim*.

safe to say with Mr. Maurice Clark, "In the case of railroads, whatever may be the ultimate tendencies, there is undoubtedly over long periods a wide divorcing, not only of unit price from unit cost, but also of total return from total cost." [1] During the continuance of that divorce the theory of monopoly which has been propounded is applicable.

To the extent to which the Ricardian conception of normal profits in the long run is appropriate, no exception can be taken to dicta above cited [2] purporting that a loss by a rate at one point tends to be recouped by a rise in rates at other points. This is, indeed, Ricardo's central doctrine that capitalists finding profits below the natural level back out of an industry in such wise that, supply being contracted, prices rise and the diminished numbers in the business thereby obtain adequate profits. How far this theory is from the facts of the railway business *judicent peritiores*. It may be remarked that the doctrine of recoupment comes with more grace from an author like Mr. Pratt,[3] who demands independence for the railways, than from those who demand that railways should be regulated, as being monopolies, with respect to actions which presuppose competition.[4]

The qualification of monopoly by competition is, as we have seen, not an unmixed advantage. Not only are certain benefits of discrimination likely to be impaired, but also certain immunities from the pressure * of raised cost. But these benefits may

[1] *Standards of Reasonableness.* [2] Above, p. 180, *et seq.* [3] *Loc. cit.*

[4] The dicta of the Inter-State Commerce Commission in this matter of recoupment are not entirely, I think, defensible upon the grounds explained in Subsection 6, above.

* The immunities here ascribed to monopoly are of two kinds. Firstly, when the raised cost which we may designate as a tax is very small, it is probable that the monopolist will not raise the price at all, the gain thereby obtainable not compensating for the trouble to himself and the disturbance to his market which the readjustment of price would occasion (*cp.* S. II. 350 and ECONOMIC JOURNAL, 1922, p. 439). Secondly, if he does raise the price to the point of maximum advantage, the rise will probably be less than what it would be in a regime of competition. For let the equation connecting $x$ the amount demanded with $p$ the price be $x = F(p)$. Then if a tax of $\tau$ per unit of commodity is imposed, we have for $\Delta p$ the consequent rise of price

$$\Delta p \frac{d^2}{dp^2} pF(p) = \tau F'(p)$$

$$\Delta p = \tau\, F'(p)/(2F'(p)) + F''(p)$$

$$= \tau\, \tfrac{1}{2}\, \frac{1}{1 + \tfrac{1}{2} F''(p)/F'(p)}.$$

In our ignorance of the sign of $F''(p)$, whether the demand-curve is concave or convex (while we know that $F'(p)$ is always negative), we are justified in assuming that $\Delta p$ hovers about $\tfrac{1}{2}\tau$. It is therefore probably less than $\tau$, what it would have been in the regime of competition. Of course $\Delta p$ *may* be greater than $\tau$, as Cournot has pointed out.

well be insignificant in comparison with the advantage attending the limitation of the monopolist's power to exploit his customers. The reader may be assisted in apprehending the nature of the limitation and the extent of the advantage by a reference to another paper.[1] The conceptions there introduced seem appropriate to the position of one who enjoys a monopolistic power of discrimination, but is deterred from using it unreservedly by the prospect of competition. He may be regarded as aiming at his own maximum profit *subject to the condition*, that his customers obtain a certain amount of benefit, namely, as much as, or perhaps a little more than, his competitors may offer. From this point of view we may discern more clearly than is usual the transition from the bad sense of the phrase "charging what the traffic will bear," to the good sense, sometimes awkwardly enough described as "not charging what the traffic will not bear." The entrepreneur in both cases discriminates prices and adjusts all manner of complicated variables to the end of securing maximum profit; but the maximum is in the one case absolute, and in the other case subject to a condition which the variables must fulfil.

To secure this beneficial result a small leaven of competition will suffice; for a reason that has been already explained, and that will be referred to again in connection with the following *second* limitation of the abstract theory.

(7) *Altruistic motives.*—The *second* modification of the originally assumed self-interested monopoly relates to the *adjective.* "Here and there better motives than egoism rule," as Professor Cohn witnesses in the context of the passage which we cited as affirming the prevalence of self-interest.[2] So Professors Johnson and Huebner, after defining the "main purpose" of railway managers to be that which we have so far assumed, added :—

"It would be as unjust as inaccurate to say that philanthropical and social motives are not also influential." [3]

More explicitly they observe :—

"The railway official . . . may no longer, nor does he, consider himself merely as the officer of a private business corporation. He realises that he holds a dual position as the servant of a corporation and as the manager of a public service." [4]

To the same effect the vigorous railway-president, E. P. Ripley, who certainly has not been influenced by any bias in favour of governmental regulation :—

[1] ζ, II. 407, *et seq.* As to the position of the partial monopolist, *cp.* S, II. 97.

[2] Above, p. 173. [3] In the context of the passage quoted above, p. 173.

[4] *Op. cit.* p. 76.

"It is needful . . . that railway managers shall see and frankly concede that they are quasi-public servants, owing a different and a higher duty to the public than almost any other business men." [1]

When asked, in the course of his examination by the Inter-State Commerce Commission, whether he would increase a discriminating rate to any extent, provided it were profitable, say by 200 per cent., he replied :—

"The advance would be too great of itself. It would be too great a shock to my sense of propriety—a shock to my sense of justice." [2]

Doubtless in the case of companies as of individuals, it is difficult to disentangle altruistic from egoistic motives. It is sufficiently accurate to say, with the wise Albert Fink :—

"Enlightened self-interest" [dictates the exercise of power] "reasonably and in a spirit of liberality." [3]

There is also to be noticed the regard for public opinion which comes to much the same practically as regard for public welfare. Among many symptoms may be mentioned the importance attached by railway men to the proportion between profits and capitalisation. Theoretically, a Railway Company is concerned to maximise the absolute amount of profits in the present (and future) without reference to the amount of capital invested in the past. Theoretically there is nothing paradoxical about President E. P. Ripley's trenchant dictum that the making of freight "has not, never did have, never ought to have any relation to the capitalisation of railroads." [4] But the opinion of the public, and, accordingly, the practice of the railways, is different.[5]

So far as altruistic motives act the doctrine of recoupment above noticed is less open to criticism. If a railway from motives of liberality has refrained from charging all that some parts of the traffic will bear, it is intelligible that when straitened at other points the Management should retract its liberality. As Mr. Pratt says, "when a railway company gets an inadequate return from one department, it is much less likely to make generous concessions in another." [6]

[1] "The Railroads and the People," *Atlantic Monthly*, Jan. 1911.

[2] *Report*, 3500, p. 351.

[3] Hepburn Commission, Vol. V. *Exhibits*, p. 87.

[4] Quoted with disapproval by the Inter-State Commerce Commission Report, 3500, p. 349.

[5] On this controversial topic Johnson and Huebner express themselves with their usual moderation.

[6] See the context of the passage cited above.

The altruistic motive need not be strong in order to be effective. An "exiguum clinamen"[1] from the direction of egoistic purpose may result in a considerable benefit to the customers. For by the theory of maxima a small decrement of profit from its maximum is apt to be attended with relatively large changes in variables connected therewith, in particular Customers' Benefit.[2]

Other corrections besides the two main ones that have now been indicated are required to adapt the rigid outline of abstract theory to human life. But these may be deferred.

[1] *Lucretius*, Book II.

[2] *See* ζ, p. 407 *et seq.*, and Index, s.v. *Maximum.*

# SECTION III

## MONEY

## SECTION III

## MONEY

# (H)

# MEASUREMENT OF CHANGE IN VALUE OF MONEY

[THIS article consists of two papers, the first and third of three Memoranda which were presented to the British Association for the Advancement of Science in the years 1887, 1888, 1889 respectively. The three were prepared by the present writer acting as Secretary of a Committee appointed for the purpose of investigating the best methods of ascertaining and measuring Variations in the Value of the Monetary Standard. The second Memorandum dealing with a special aspect of the subject is reprinted as a separate Article, the second of the present section. The first and third Memoranda being *in pari materia* are here put together. The collocation of an originally somewhat diffuse disquisition with afterthoughts which occurred two years later does not form a model of order and unity. The composition is like the " scene of man," according to Pope, " a mighty maze "; but that it is " not without a plan " may appear from the following *résumé*.

The " natural method of calculating a measure of change," in the phrase of Mr. Flux (*Journal of the Statistical Society*, 1921, p. 175), is to determine the change in the money value of the articles consumed by the population under consideration. This standard is considered in the first section of the first Memorandum. The simplest form of this standard is " the comparative money-cost of a fixed schedule of articles " (Flux, *loc. cit.* *Cp.* N, below, p. 396). The most refined form of this standard compares the amounts of money required to procure the same *satisfaction* at different epochs (*Cp.* Sidgwick, *Political Economy*, Book I., ch. ii. § 3; Bowley, *Journal of the Statistical Society*, 1921, p. 351). Over against this Consumption-Standard is the Production- or Labour-Standard, which compares the amounts of money procured by the same " Real Cost " in the sense of effort-and-sacrifice. This standard is propounded in the last

section of the third Memorandum. Quantity of labour may not be a very distinct idea; as Adam Smith says, "the greater part of people understand better what is meant by a quantity of a particular commodity." There is, however, a very distinct difference between this and the first standard. Thus, if gold prices fall through increased production of commodities, as perhaps in the eighties of last century, according to the first standard there may appear a serious appreciation demanding correction; according to the second standard there may be no change in the "real value" (Marshall and Ricardo) of money, gold is behaving very well. These two standards may be regarded as species of a genus which may be described as subjective, or personal, in contrast with objective standards which are less directly adapted to definite human purposes. Such is the character of the "Indefinite Standard" consisting of a mere average, as propounded in Section VIII. of the first Memorandum, where it will be observed that some hypothesis involving sporadic dispersion of prices—before (with respect to standards of the first genus) repeatedly stated not to be implied—is now for the first time introduced. This may be considered as the first species of the objective genus. A second species is presented when we consider not only the fact of an average change in prices, but also as its cause the change in the relative quantity (and velocity) of circulation. This species may be identified with the second method of the third Memorandum; connected in 1889 with the name of Foxwell, and subsequently elaborated by Professor Irving Fisher. Within this species there is a variety which not only connects the change in prices with change in the quantity of circulation, but also considers as the cause of that cause change in the quantity of gold, or other primary money on which the circulation (cheques and notes) is based. The indefinite standard may also be divided into species of which one is irrespective of the quantities of commodities, the other takes account of quantities. The second species includes the variety just now mentioned (second part of Section IX., First Memorandum), and more generally cases in which the conditions of a perfect market are not realised in such wise as to render the first species appropriate (first part of Section IX.). A cross-division of the indefinite standard is formed by the use of different averages; in particular the Arithmetic Mean, the Median, the Geometric Mean and the Mode, the familiar four to which the character of objective and not directly adapted to special purposes principally appertains.

The genus index-number of prices may be defined so as to

include the measurement of differences between different *places* in the value of money (p. 290).

Another cross-division of the genus is formed by the limitation of the population or district to which the computations refer. The index-numbers examined on Sections III.–V. of the third Memorandum may perhaps be considered as thus referring to a particular interest, foreign trade.

Or these, like several of the remaining sections, may be considered as dealing with imperfect measures, symptoms and indications of one or other of the index-numbers above defined. Section III. of the first Memorandum is thus related to the Consumption-Standard of Section II. Section IV. and V. of the first Memorandum may be subordinated to the Production-Standard of Section VII., third Memorandum. Section I. of the second Memorandum bears a similar relation to Section VI. Section I. of the second Memorandum might also be treated as an imperfect substitute for Section II. of that Memorandum (an abridgment in which account is not taken of the pull upon currency exercised by the repeated sale of a commodity). It is a question whether the Capital Standard proposed by Professor Nicholson should be treated as subsidiary or as a new substantive index-number.

There is finally an index-number subordinated not to one or other of the ends defined, but to several of them; adapted to secure the maximum of utility, regard being had to the different kinds of advantage described in the different sections. Such is the purport of the "mixed modes" in Section X. of the first Memorandum corresponding to Professor Wesley Mitchell's "general purpose" index-number (see **N**, below, p. 387).

It may be asked, How does the indefinite standard enter into a compound of this sort? The answer that it contributes the important attribute of a "true mean" (Section VIII., First Memorandum, p. 233), a "good average" (*Journal of the Statistical Society*, 1923, p. 572). Such is the purport of that compromise between the principles of the Consumption-Standard and the more objective species which the British Association Committee seemed to sanction (Section X. of First Memorandum). The weighted Arithmetic Mean prescribed by the Committee would be proper in the absence of any hypothesis as to the dispersion of the data. But the formula may be used with more confidence when the hypothesis of sporadic distribution is known to be realised.

For further elucidations of the Memoranda the reader is

referred to two papers dealing with hostile criticism, viz. *The doctrine of Index-numbers according to Mr. Correa Walsh*, ECONOMIC JOURNAL, 1923, and the *Calculation of Index-numbers by Mr. Correa Walsh*, "Journal of the Statistical Society," 1923. Mr. Walsh makes a rejoinder in *The Quarterly Journal of Economics*, May, 1924.]

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## FIRST MEMORANDUM

### CONTENTS.

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## Introductory Synopsis.

The object of this paper is to define the meaning, and measure the magnitude, of variations in the value of money. It is supposed that the prices of commodities (including services), and also the quantities purchased, at two epochs are given. It is required to combine these data into a formula representing the appreciation or depreciation of money.

It will appear that beneath the apparent unity of a single question there is discoverable upon a close view a plurality of distinct problems. Many different branches have been traced, and the number might be largely increased if every bifurcation were followed out to its logical end. But it is not to be supposed that the innumerable ramifications which a formal logic might be able to distinguish would all repay cultivation. The most rigorous analysis may be content with a dozen distinct cases; and for the purpose of an introductory summary these may be reduced under a still smaller number of headings.

To one taking a general view of the subject there stand out four main types, four modes of measurement distinct in idea and definition, though occasionally coincident in practice. The *first* sort of measure is based upon the change in the prices of finished products, the object being to find, or rather show how to find at any future time, a ratio or *Unit* such that the creditor in the future receiving as many Units as he at present receives

pounds may derive as much advantage in the way of consumption then as now. The *second* sort of measure is based upon all the articles which trade deals with, the object being to find a Unit such that the debtor in the future, paying as many Units as at present pounds, may not be more hampered in his business then than now. There is *thirdly* the measure of that appreciation which it is the object of bimetallism and similar projects to correct? The *fourth* sort of measure is required not so much for any particular practical object as for the more general purposes of Monetary science, to interpret the past and forecast the future.

Let us add a few words on each of these methods separately, to explain more clearly either the means adopted or the end proposed, or how far those means are conducive to that end.

(1) The general principle of the first method may be embodied in slightly different rules, of which the following two may claim to be the best. ($\alpha$) In order to ascertain the change in the value of money between two epochs, find the national [1] expenditure per head upon finished products or articles of consumption (including unproductive services) at each epoch. The ratio of the new to the old expenditure is the required measure of depreciation, or Unit. Otherwise ($\beta$) thus (the general principle being interpreted somewhat differently): Find the quantities of each article consumed at the two epochs, and take the mean of each couple. Multiply each of these mean items by the old price of the corresponding article and add together these amounts. Proceed similarly with the new prices. The ratio of the latter sum to the former is the required Unit. There are other formulæ, in all more than half a dozen. But there is not much to choose, among them. And the exercise of a choice may exceed the powers and province of the writer.[2]

The advantages of rendering money a steady measure of value-in-use would be considerable wherever there may be violent fluctuations of general retail prices.[3] Such oscillation in the purchasing power of money intensifies the ups and downs of Fortune—so trying both to the sentient and the moral nature of man. The disturbance superadded by a bad currency might be annulled by a corrected standard. The honest labourer would not be cheated of his reward by miscalculations of the value

[1] See below p. 223; and p. 213.

[2] To choose between the first of the rules just given and the second is beyond the scope of this paper.

[3] The advantages of a "Tabular Standard of Value" have been pointed out by many writers. See Jevons, *Currency and Finance*, p. 122, and the references given in the note.

of currency. Those who had laid out their lives upon the faith of a fixed income would not be disappointed of their just hopes. The provision for the widow and the orphan would be more secure. The endowments of learning would preserve that constancy of competence which is favourable to the cultivation of the liberal arts.

These great advantages seem capable of being largely realised. For it is shown by statistics, such as those of Engel [1] and the Massachusetts Labour Reports,[2] that there is considerable constancy in the budgets of family expenditure. Thus in Massachusetts in 1885 the average workman spent out of 100 dollars 29·5 upon groceries, 19·7 upon provisions, 4·3 upon fuel, and so on. Suppose a Unit or corrected dollar continually equivalent to the amounts of groceries, provisions, fuel, etc., which in 1885 were respectively purchased for ·295, ·197, and ·043. There is reason to believe that such a Unit would afford a tolerably constant sum of satisfaction to the Massachusetts working family. But we cannot expect an equally perfect measure, when we construct a Unit, not for a class, but a nation.[3]

(2) The desirability of prescribing separately for different interests is even more strongly brought before us when we consider the second of the methods above defined. It purports to be a *sliding scale* for general use, adapted to all trades. But what fits all indiscriminately cannot fit many exactly. We may say of such a project what Steuart says of a certain "ideal standard," that it is "acting like the tyrant who adjusted every man's length to that of his own bed, cutting from the length of those who were taller than himself, and racking and stretching the limbs of such as he found to be of a lower stature." It would not be unreasonable, however, to construct beds of different sizes, adapted to the average height of markedly different classes of persons, say little boys and men. Similarly, when average prices have largely varied, a scale sliding with the average variation, however imperfectly fitted to particular trades, may be suitable to industry as a whole. The illustration shows the spirit in which our calculation should be performed. What should

[1] *Volkswirtschaftliche Zeitfrage*, Heft 24. *Inst. Natl. de Statistique*, N. 5.

[2] For 1885. See also Young, *Labor in Europe and America*.

[3] Professor Foxwell writes: "I think it would also for many purposes be extremely convenient to have an index-number, or numbers, indicating the altered purchasing power of selected amounts of consumers' incomes, estimated in the corrected standard. I mean that having first determined, by our principal standard, the corrected value of £1 for the given year, we should then find the alteration in the purchasing power of the new standard £1 for different incomes: *e. g.* for incomes of £50, £100, £200, £500, £1,000, and £10,000."

we think of an upholsterer who, having to construct different types of bed, should invoke the aid of the British Association Anthropometric Committee nicely to determine *l'homme moyen* for different ages? The labours of that committee would not be more misspent than ours, if we attempted in framing a universal sliding scale to determine the ideally best *weight* for each item entering into the combination. Almost any combination of the more important articles of trade is likely to be equally imperfect and equally serviceable (see p. 228).

The advantages aimed at by this method may be presented under two aspects. That steady secular decline of prices which, according to many eminent writers, is a cause of the depression of trade, might be corrected. The advantages offered by bimetallists would be attained. There might be also another benefit which not even bimetallists venture to promise. The sudden violent oscillations in general prices, occasioned by the derangement of credit, would be arrested. For, as the supply of money to meet debts became deficient, the demand for money to meet debts would proportionately dwindle; the amount of debts in "standard" currency inversely varying with the value of metallic money.[1] The hunger for gold would be less felt just as the means of satisfying it were less abundant. Heretofore a contraction of currency has acted like an atmospheric depression in the physical world. The drain and rush of the medium has produced a storm. But in the new commercial Cosmos, equilibrium between debts and currency being continually preserved, the stormy winds of Panic will have ceased to blow. Hitherto the relation between liabilities and currency has been that of a continent to the ever-changing level of the sea. Each ampler tidal wave has rendered harbours unserviceable, and dislocated trade, and strewn the shore with wrecks. But the latest invention of science is a sort of *floating dock*, which shall rise with the flood and sink with the ebb, so that the argosies of commerce may be safely landed, whatever the level of the transporting medium.

These are fascinating images, ideal possibilities, which the sober thinker may entertain while he is conscious how remote and uncertain is the realisation; how numerous the difficulties and objections. Perhaps the new organisation of the money market would develop new varieties of roguery. Certainly complications would arise between liabilities to the foreigner

[1] This action is well exemplified in the plan proposed by William Cross, that the standard should vary *per saltum*; a correction being made as often, say, as money was appreciated (or depreciated) by 3 per cent.

expressed in gold, and engagements with the home trader expressed in the adjusted currency. It is alleged, too, that the business of banking would be impeded. In fine, the common sense of business men appears opposed to the scheme; and, on the question what is at present practicable and what not, the opinion of practical men, even unsupported by reasons, is conclusive.

(3) The third inquiry is, What is the appreciation (or depreciation) which it is the object of bimetallism and similar projects to correct? What is that mean (or function) of prices which the bimetallist would desire to keep constant? Of course, if prices varied all in much the same ratio, like the lengths of shadows with the advancing day, the answer would be very simple. That ratio is the required measure. But suppose that one large category of prices is pretty uniformly elevated, while another is *en bloc* depressed; we desiderate a measure which, like the two preceding, may be independent of the particular hypothesis that there has been a uniform average price-variation all over the field of industry.*

It is to be observed that the Unit required for this purpose cannot be restricted to a particular geographical or industrial area. Rather the averaging must be extended over the whole system of countries in monetary communication—that is, over the greater part of the civilised and uncivilised world.

(4) When we consider the next type, the fourth definition of our problem, there once more is pressed upon us the expediency of limiting the area of markets over which our measurement is to extend. It may be doubted whether a standard based upon the variation of all prices indiscriminately would—abstracted from some definite particular purpose such as those contemplated in the preceding paragraphs—be of much scientific use. It would be like taking the mean barometric pressure over a large continent. It is more useful to observe the variation of pressure at particular stations, in order to predict what changes will be propagated to neighbouring regions, what storms are coming. Suppose, for the sake of illustration, that at any station the reading of a single barometer was not sufficient to give the true pressure; that each instrument was liable to a proper disturbance over and above the general atmospheric change. The heat or cold, for example, of different situations might cause a misleading

* I have omitted two sentences identifying the desiderated Mean with one of the two preceding or some cross between them. Rather, the formula which the writer is here feeling after is to be identified with the Currency Standard defined in Section 2 of the Third Memorandum (below p. 261, *et seq.*).

expansion or contraction of the mercury. On such a supposition it might be proper, in order to measure the pressure at any station, to take a mean between the readings of several barometers. Upon well-known hydrostatical principles, no particular importance, other things being equal, would attach to the reading of the barometer which contained a particularly large mass of mercury.

These conceptions appear appropriate to our problem. We should demarcate a certain region of industry, and estimate in terms of that special group of articles an index-number indicative of changes which are likely to become general. The zone of observation most suitable to our purpose would probably be as it were the coast-line of trade, those articles of world-commerce which are most sensitive to changes propagated from abroad. In taking such a mean of observations the "weights" are not necessarily proportioned to the masses of commodity. *Primâ facie* and in the abstract pepper may afford as good an index as cotton (see p. 243 and context). The writer has given rules for taking the mean of these observations. But he is aware how difficult it is to define the proper zones; how hardly susceptible of perfection is the science of monetary meteorology.

Contemplating all these types we discern a property common to most of them, the desirability of treating separately selected interests, rather than operating upon all commodities indiscriminately. To construct such partial measures does not seem to be the business of this Committee, or at least this Memorandum. We may, however, hope that our theoretical diagnosis of different purposes may be of use to those who undertake the more practical task of prescribing for different interests.

## SECTION I.

### *Description and Division of the Problem.*

The business of this Committee is to measure a fact, not to speculate about its causes or consequences. Should a fall in the value of money have occurred we need not trace that phenomenon to its sources. Whether it takes its rise on the side of the precious metals or of commodities—whether, in Dr. Johnson's phrase, it is the pence that are few or the eggs that are many—it is not our part to determine. The consequences of the change are equally outside our province. It is open to us to hold with Hume that, when prices are rising owing to the influx of money, "everything takes a new face; labour and industry gain life." With General Walker we may predicate the converse attributes

of falling prices. Or we may accept Professor Marshall's [1] qualified, or Mill's [2] negative, statement of those effects. We have to leave speculation and apply ourselves to measurement.

But, while we are not called upon to decide such controverted questions, we cannot be as indifferent to the decision as might at first sight have appeared. For it is only in the simpler kinds of measurement that the metretic art can be entirely divorced from theory about its subject-matter. To measure the height of a man we do not require a knowledge of anthropology. We may even ascertain the mean stature of a nation without much special knowledge. But difficulties arise when we have to do not with one attribute, such as *height*, but with two (or more) attributes: for instance, the masses and velocities of a system of bodies. Take the simple case of a number of heavy particles at rest, and suppose that different velocities are imparted to the different particles between two given epochs. It would not be very easy for one coming fresh to the study of mechanics so to define his confused general idea of the *change of motion* which had occurred as to be able to express it in terms of the data: namely, the masses, say $M_1$, $M_2$, etc., $M_n$, and the imparted velocities (which, in order to minimise difficulties, we will suppose all in the same direction) $V_1$, $V_2$, etc., $V_n$. It is plausible to say that the problem is purely statistical, that we seek a merely objective result. The difficulty is that any combination—at least, any symmetrical combination—of the data is in a sense objective. We must call in mechanical science to determine what combinations are worth forming and what are insignificant. Consider the two combinations $M_1 V_1^2 + M_2 V_2^2 + \text{etc.} + M_n V_n^2$ and $M_1^2 V_1 + M_2^2 V_2 + \text{etc.} + M_n^2 V_n$. *Prima facie*, these are both equally "objective," and they seem equally simple. But while the former (the expression of energy) constitutes a spell for opening all the secret chambers of Nature, the latter could only be significant on some very peculiar hypothesis, for some very out-of-the-way purpose.

Similarly, in the problem before us we have to combine two sets of data, the prices of different articles and the quantities thereof. Indeed, our problem is rather more complicated. We may have to take account of a third attribute, the *quality* or species of wares; to consider, for instance, whether the price and quantity of labour or of materials shall enter *pari passu* and symmetrically into that combination of our data which we desiderate.

In order to discover the principle on which this combination

[1] *Third Report on Industrial Depression*, Appendix C, Vol. II. p. 422.

[2] *Political Economy*, Book III. chap. xiii. § 4.

is to be effected, we may be led into the most perplexed regions of monetary science. We are brought against the question, What is the relation between the amount of money in a country and the general scale of prices?—the question which has been called by a distinguished authority [1] "one upon which the most contradictory opinions have been expressed by economists of reputation." And even where there are no fundamental differences of theory, yet practice may vary according to the practical end in view. Some may aim at the construction of a tabular standard, adapted only to contracts extending over a long period of time; others may desiderate a more flexible standard, which may mitigate the effects not only of the secular, but also of the more [2] transient variations in the value of money; others may seek only an index of the future course of prices—a sort of monetary barometer.

There are therefore many methods—not one method—of "measuring and ascertaining variations in the value of money." The path which we have to investigate has many bifurcations. To decide at each turn which is the right direction is either impossible, or at least presumptuous. It is impossible when both ways are right, directed to different but equally legitimate ends. And, even where there must be a right and wrong, it is not becoming here to pronounce upon points controverted by high authorities. The course adopted is to trace separately the alternative paths, indicating the difference without expressing a preference.

In this memorandum it is proposed to distinguish the various cases of the general problem, and to construct the formula appropriate to each case. The numerical determination of the quantities which enter into the formulæ—both the compilation of the proper figures from explicit statistics, and, where these are wanting, the more speculative arts of inferring unknown prices and amounts from imperfect data and indirect indications—these parts of the subject are not treated by the present writer. They may be considered in a future Report of the Committee and in separate memoranda contributed by other members.

The delicate subdivisions of the subject are exhibited in the annexed diagram by means of a regular *logical tree*.[3] In examining this tree of knowledge we shall give priority to the branches

[1] General Walker in his *Money*.

[2] As Professor Marshall hopes; *Contemporary Review*, March 1887.

[3] As logical and genealogical trees for the most part, like the trees in the poet Parnell's *Hermit*, "depending grow," it may be as well to point out to the reader that our tree, like those cultivated by some of the earlier logicians, is trained *upwards*.

PROBLEM OF MEASURING CHANGE IN THE VALUE OF MONEY.

- A. Solution irrespective of hypothesis as to the cause of the data.
  - B. *Quæsitum*, the construction of a standard for deferred payments.
    - C. Standard required to be constantly equivalent to the same quantity of valuables.
      - D. Constituents of the standard corresponding to the items of national consumption.
      - d. Constituents of the standard determined on some other basis than national consumption.
    - c. Standard required to vary on the principle of a sliding scale.
      - D. Constituents of the standard corresponding to the items of national consumption.
      - d. The standard determined on some basis other than national consumption.
        - E. The standard varying with national income.
        - e. The standard varying with national capital.
  - b. *Quæsitum* other than the construction of a standard for deferred payments.*
- a. Solution based upon an hypothesis as to the cause of the data.
  - F. Irrespective of quantities of commodities.
  - f. Account being taken of quantities of commodities.

* A variant definition of $b$ is offered by the phrases used in the remarks now prefixed to this article—"not adapted to any special or definite purpose." So understood, the attribute $b$ would often occur with, could hardly occur without, the attribute $\bar{a}$, implying the existence of a true mean or good average.

on the left. As soon as we have reached the definition of each ultimate species we shall add its properties—the treatment adapted to that particular case. We shall not only trace out the form of each branch, but also gather the fruit at its extremity, before we go on to the branch nearest on the right.

The whole subject is first divided according as the method adopted is (A) irrespective of any hypothesis as to the cause of the price movements or (a) is based on some such theory. Deferring the treatment of the latter case (a) we proceed to divide the former according as (A B) the practical purpose in view is to construct a standard or "Unit" for deferred payments, or (A b) some other purpose. Postponing the latter case, we may complete the definition of the former by explaining that the Unit (a term borrowed from Professor Marshall's recent article in the *Contemporary Review*), as used here in a general sense, means a sum of money estimated to be equivalent at present (or at some future time) to what a Unit of money, say a pound, was worth at some past time: in such wise that it may be just or expedient for debtors to pay, and creditors to receive, as many Units now (and from time to time) as they contracted to pay and receive pounds at the initial epoch. The general idea of a Unit may be specialised according as it is required that (A B C) the Unit should constantly be equivalent to the same quantity of valuables, or (A B c) that it should not represent a constant purchasing power, but one varying with the means of debtors, after the manner of a *sliding scale.* Lastly the kinds and quantities of the valuables entering into the Unit may either (A B C D) correspond to the items of national consumption, or (A B C d) may be selected on some other principle. In this arrangement priority does not import any preference.

## SECTION II.

*Determination of a Standard for Deferred Payments; based upon the items of national consumption; calculated to afford to the consumer a constant value-in-use; no hypothesis being made as to the causes of the change in prices.* (A B C D.)

According to this arrangement, the first case for which we have to prescribe is where, apart from any hypothesis as to the cause of the movement of prices, we want to construct a Unit adapted to deferred payments, and where it is required that the Unit should be constantly equivalent to the same amount of valuables, the kinds and proportions of the valuables corresponding

to the items of the national expenditure. Upon reflection it will be found that the last attribute involves, or is deduced from, some such condition as the following—that the *advantage* which an average person derives from the expenditure of a Unit should be constant.[1] *

From this condition, owing to the unequal consumption [2] of different individuals it follows that the precision of our calculation cannot be great. That is to say, we cannot be certain that between considerable limits some other ratio than the one which we have chosen would not be as good as the one which we have chosen.

It may be worth adding that even if we could suppose that all commodities were consumed in the same proportions by all

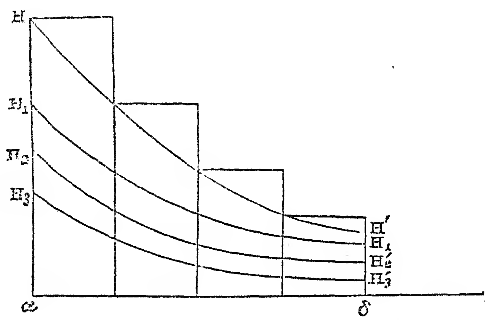


FIG. 1.

individuals, yet the mere difference in the size of fortunes and of debts would introduce an inaccuracy. To show this let us first suppose that all fortunes would be equal but for the payment of debts; and let us represent the average amounts of commodities consumed by the height of the columns in the annexed diagram, the divisions of the horizontal line being equal. Now suppose a person, from being a consumer of the average amount of each article, becomes debtor to the extent of a certain sum, expressed in *Units* [3] of tabular standard. Theoretically he would retrench something of his expenditure on each article, contracting as it were the *margin of final utility*. He might thus fall back upon

[1] *Cp.* Horton, *Silver and Gold*, chap. iv. "In the average annual consumption of provisions . . . we should have at least fixed a definite portion of utility. . . . By enlarging the sphere of consumption on which to base the average . . . we still more nearly attain a measure of the value of Money."

* *Cp.* references to Sidgwick and Bowley given above, p. 195.

[2] *Cp.* Professor Marshall, *Industrial Conference.*

[3] The term *Unit* is here employed in the sense proposed by Professor Marshall, *Contemporary Review,* March 1887.

the curve $H_1H'_1$ instead of the original boundary. And if his debt increased he might have to fall back upon an interior frontier, the next *isohedone*, as we might call this family of curves. Conversely, in the case of a creditor. Now, in order that our standard should be applicable to debts of various sizes it is virtually assumed that the ratio $HH_1 : H'H'_1$ is the same as $H_1H_2 : H'_1H'_2$, and so on for other columns and curves. But this assumption is without evidence, or rather contrary to evidence. Or, if it be held sufficient that the standard should represent the utility corresponding to the *average* debt, still even for this purpose our method of determining the proportions (by the totals consumed) is arbitrary. *A fortiori* when we admit all kinds of inequalities of fortune and other irregularities. Thus it may plausibly be contended in virtue of the analogies of Fechner's law that, where the total wealth of a people has increased, an equal quantity of utility is represented by a larger quantity of wealth.[1] In this

[1] The standard defined in this section, the Consumption Standard as it may be called, appears to be particularly appropriate to the case in which National Wealth is regarded as a constant quantity. Otherwise there is apt to arise a divergence between two attributes which we have hitherto assumed to be conjoined, namely, the condition that the Unit should be constantly equivalent to the same quantity of valuables, and that it should afford, on an average at least, the same quantity of value-in-use, the same "Final Utility." For, according to the *Law of Diminishing Utility* (expounded by Laplace, Jevons, and others), the same increment of means tends to afford a smaller increment of advantage when the fortune to which addition (or from which subtraction) is made is ampler. If, then, National Wealth increasing, the average fortune becomes larger, the Unit which is equivalent to the same quantity of things will no longer correspond to the same quantity of advantage. The average scale of living being higher, the same amount of goods will not appear of the same importance to the average consumer [see *Recent Writings on Index-numbers*, below, J.] Accordingly, in such a case we must make a choice between the following two conditions for the definition of our Standard or Unit. The first condition is that the Unit should constantly be equivalent to the same quantity of valuables. Or since, agreeably to the views here adopted, quantity of valuables cannot be in general defined irrespective of subjective considerations, it might be more philosophical to lay down as the first condition that the Unit should constantly afford the same quantity of utility; *abstracting the change of National Wealth*, supposing that the fortune of the average consumer remained constant. The alternative condition is that the utility afforded should be constant, that circumstance *not* being abstracted. As a matter of nomenclature, it seems better to restrict the symbol C, the term *Consumption Standard*, to the former definition. The latter arrangement may be regarded as a variety of the genus *sliding-scale*, designated by c.

Dr. Julius Lehr, in the important contribution to our subject made in his *Beiträge zur Statistik der Reise* (Frankfort, 1885), seems to assume the proposition that the utility derived from wealth at both the compared epochs is the same; or at least that the final utility at each epoch is the same, or rather a quantity of the same order. For he takes as the measure of the importance of an article the number of *Genusseinheiten* afforded by its consumption. Now, Dr. Lehr's *Genusseinheit* and Jevons' *Final Utility* are quantities of the same dimension.

case Method A B c D (explained below) might be the legitimate deduction from the principle on which we here suppose method A B C D to depend.

It is important to realise how loose is the character of the calculation even under the most favourable conditions. To expend meticulous care in determining our weights when our balance is thus rough is nugatory. It is taking care of the pence and leaving the pounds to take care of themselves, a course dictated rather by proverbial than practical wisdom.

It follows from the condition above stated that the frequent resale of an article (such as cotton) forms no reason (with reference to the purpose of Section III) why it should be counted more than once. Nor should materials, as distinguished from finished products, be counted, or only as representative of finished products. Upon the same principle the price of stipendiary labour [1] (domestic wages and many professional payments) enters in as an independent item; but the price of industrial labour (ordinary wages) only as representative, and in the absence, of the finished products.

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A hundredweight of diamonds, say, affords so many times more *Genusseinheiten* than a hundredweight of iron, as the Final Utility of the former is greater than the Final Utility of the latter. To determine the number of *Genusseinheiten* conferred by (the objective unit, *e. g.* hundredweight, of) each species of article, Dr. Lehr in effect takes the mean of the Final Utilities at each epoch. Now he who takes a mean assumes that the quantities of which he takes a mean are of the same order.

[1] The exclusion of "services" as distinguished from material commodities has been maintained on the ground that so-called "unproductive" labourers are paid out of the proceeds of productive industry. The money which we expend on singers and dancers finds its way to butchers and bakers. To include in the National Inventory the outlay on Singing and Dancing as well as the total expenditure on Bread and Meat is therefore to count the same portion of wealth twice over. And no doubt this remark is relevant, where the object is to measure the quantity of Wealth defined as something *material*. But for the present purpose would it not be theoretically as reasonable to omit Bread and Meat and base our standard exclusively upon the price of theatrical entertainments and such like, upon the ground that what we pay to the butcher and baker finds its way to the Music Halls which they frequent? "No," it may be replied, "for a good part of their income must be expended on material necessities." Well, but by parity a good part of the wages of "unproductive" labour may be expended on immaterial utilities. What is earned by teaching literature may be spent in tickets for the opera. Theoretically it is as arbitrary to exclude altogether immaterial utilities as it would be to include nothing but them. The difference between the two errors is only one of degree and practical importance. As a matter of fact in the existing world, of the two defective methods the less imperfect is that which includes material, and excludes immaterial, utilities. But the converse might be true in some happy island, where the material necessities of life were obtained almost for nothing, and the principal monetary transactions were constituted by the exchange of mutual services.

In constructing the formula for combining the quantities and prices thus defined, we may first distinguish the abstract and ideally simple case in which exactly the same quantity of each article is consumed at the two epochs. In this case the method of procedure is that indicated by Professor Sidgwick in his *Political Economy* (Book I. chap. ii. § 3): "Summing up the amounts of money paid for the things consumed [1] at the old and the new prices respectively," and [to find the value of the Unit at the later epoch] dividing the latter sum by the former.

A difficulty arises when we introduce the concrete circumstance that the quantities consumed at the two epochs are not the same. We might distinguish two grades of this deflection from the abstract ideal: (I) where the interval of time between two revisions being very small the variations in the amounts consumed are slight, *differentials*, we might call them; and (II) *integral* or considerable changes which occur in the course of a long interval of time.

I. The method of procedure in the first case may thus be symbolised: Let $\alpha$, $\beta$, $\gamma$, etc., be the quantities of commodities consumed [1] at the initial epoch, and $\alpha'$, $\beta'$, $\gamma'$, etc., at a subsequent epoch; it is assumed that $\frac{\alpha'}{\alpha} = \frac{\beta'}{\beta} = \frac{\gamma'}{\gamma} = \text{etc.} = 1$ nearly. And similarly for a second subsequent epoch $\frac{\alpha''}{\alpha} = \frac{\beta''}{\beta} = 1$ nearly. Upon these assumptions several methods of determining the Unit present themselves. Let us designate the prices at the initial epoch by $p_\alpha$, $p_\beta$, $p_\gamma$, etc., and at a subsequent epoch $p_\alpha'$, $p_\beta'$, $p_\gamma'$, etc. Then,

(1) We may take the type which first presents itself upon Professor Sidgwick's view of the problem, viz.—

$$\frac{\alpha p'_\alpha + \beta p'_\beta + \text{etc.}}{\alpha p_\alpha + \beta p_\beta + \text{etc.}}.$$

This method is (in effect) adopted by Mr. Sauerbeck for years earlier than 1866–77 (*Journal of the Statistical Society*, 1866, pp. 595–613).

The method is also exemplified by Mr. Giffen's retrospective estimate of the change in the value of money between 1873 (and 1883), and *earlier* years (Report on Prices of Exports and Imports, 1885, Table V.).

[1] Agreeably to this definition the prices on which the Consumption Standard is based should theoretically be the prices paid by consumers—retail prices. For *this* purpose wholesale prices are to be employed only in the absence of the proper statistics, as an index of prices paid for the finished products—a very imperfect index, as Dr. Scharling, in his excellent paper on retail prices, and other authorities have shown.

(2) The next type, also given by Professor Sidgwick,[1] is the converse of the first, viz.—

$$\frac{\alpha' p'_\alpha + \beta' p'_\beta + \text{etc.}}{\alpha' p_\alpha + \beta' p_\beta + \text{etc.}}.$$

This method is exemplified by Mr. Giffen in his Table IV. (Reports 1881, 1885), by Mr. Mulhall, and by Mr. Sauerbeck (for years after period 1867–77), (*Journal of the Statistical Society*, 1886, p. 595).

(3) The third type is a mean between the first two, viz.—

$$\tfrac{1}{2}\frac{\alpha p'_\alpha + \beta p'_\beta + \text{etc.}}{\alpha p_\alpha + \beta p_\beta + \text{etc.}} + \tfrac{1}{2}\frac{\alpha' p'_\alpha + \beta' p'_\beta}{\alpha' p_\alpha + \beta' p_\beta + \text{etc.}}.$$

Professor Sidgwick has suggested and remarked upon this procedure in a note. It has been noticed also by Drobisch.

(4) The next type is also a mean :—

$$\frac{\frac{1}{2}(\alpha + \alpha') \times p'_\alpha + \frac{1}{2}(\beta + \beta')\, p'_\beta + \text{etc.}}{\frac{1}{2}(\alpha + \alpha')\, p_\alpha + \frac{1}{2}(\beta + \beta')\, p_\beta + \text{etc.}},$$

suggested independently by Professor Marshall and the present writer.

(5) The next type is one adopted by Mr. Palgrave :—

$$\frac{\alpha' p'_\alpha \times \dfrac{p'_\alpha}{p_\alpha} + \beta' p'_\beta \times \dfrac{p'_\beta}{p_\beta} + \text{etc.}}{\alpha' p'_\alpha + \beta' p'_\alpha + \text{etc.}}.$$

(6) The sixth type is that which Mr. Giffen has employed in his Table III. Put ${}_{,}\alpha$ and ${}_{,}p_\alpha$ for the quantity and price of the first commodity in 1875 (or other year selected as representative). Then for the increase in the value of money in the year whose symbols are $\alpha'$, $p'_\alpha$, as compared with year $\alpha$, $p_\alpha$, write :—

$$\frac{{}_{,}\alpha{}_{,}p_\alpha \times \dfrac{p'_\alpha - p_\alpha}{p_\alpha} + {}_{,}\beta{}_{,}p_\alpha \times \dfrac{p'_\beta - p_\beta}{p_\beta}}{{}_{,}\alpha{}_{,}p_\alpha + {}_{,}\beta{}_{,}p_\beta + \text{etc.}}.$$

The expression for what we have called the Unit is found by adding *unity* to the above $\left(\text{substituting } \dfrac{p'_\alpha}{p_\alpha} \text{ for } \dfrac{p'_\alpha - p_\alpha}{p_\alpha}\right)$.

(7) Next we may place the formula of Drobisch, of which the principle is to compare the price at different epochs of an objective unit, such as a hundredweight, supposed to be made up of all sorts of articles in the proportion in which they enter into national

[1] See the passage above referred to.

consumption. In our notation the formula (for what is here called the unit) becomes

$$\frac{\alpha' p'_\alpha + \beta' p'_\beta + \text{etc.}}{\alpha' + \beta' + \text{etc.}} \div \frac{\alpha p_\alpha + \beta p_\beta + \text{etc.}}{\alpha + \beta + \text{etc.}}.$$

(8) Last, but not least, either in respect of bulk or of theoretic weight, occurs the formula of Dr. Julius Lehr (referred to above, p. 211), of which the principle is to compare the price at different epochs of a *pleasure-unit*, or unit of final utility. The formula may be thus conveyed in our notation :—The mean " Genusseinheit," or *final utility*, of the first commodity is $\frac{\alpha + \alpha'}{\alpha p_\alpha + \alpha' p'_\alpha}$. Of such units these came into consumption, $\alpha$ at the first epoch, and $\alpha'$ at the second. Now sum up all the *Genusseinheiten* for all the commodities which came into consumption at the initial epoch, and divide the national expenditure ($\alpha p_\alpha + \beta p_\beta +$ etc.) by the sum of *Genusseinheiten.* Thus you have the average price at the initial epoch of a *Genusseinheit ;* say $P_1$. Similarly determine $P_2$ for the posterior epoch. Then $P_2 \div P_1$ is the required unit.[1]

Of these methods it may be remarked that the first four seem to have an advantage over the remaining two, in that the former make no assumption as to the extent of the change of price, while the latter proceed on the supposition that those changes are small. The fifth method seems to assume that we may write for $p'_\alpha$ $p_\alpha(1 + \Delta'_\alpha)$, where the second powers of $\Delta'_\alpha$ are negligible. And similarly in the sixth method we must be allowed to write for ${}_\prime\alpha{}_\prime p_\alpha$ ${}_\prime\alpha p_\alpha(1 + {}_\prime\Delta_\alpha)$, where ${}_\prime\Delta_\alpha \times \Delta'_\alpha$, ${}_\prime\Delta_\beta \times \Delta'_\beta$, etc., are negligible. No doubt, when we grant the steadiness of the

[1] With regard to the formula proposed by Dr. Lehr, the present writer agrees with the criticism expressed by Professor Lexis in a recent number of Conrad's *Jahrbuch.* The received formulæ and Dr. Lehr's formula are equal as touching their theoretical validity; but the former (including our A B c D) have the advantage of practical simplicity.

Dr. Lehr's treatment of the *variables* as distinguished from the formula also calls for remark. His object being to discover how far the power of money to purchase *Genusseinheiten* has varied, it is not quite clear why he should insist on including wages, the wages of ordinary industrial or productive labour as well as of stipendiary services, among the data. Do we not take sufficient account of productive labour when we take account of the finished products? *Either,* but not *both,* these items should figure in the expression of our Unit.

One more remark seems called for in justice to the reader whom our notice of this work may have attracted. He must not be discouraged by the opening paragraphs, which are both extremely obscure and not directly relevant to our present purpose. The general reader is advised to begin at p. 10 (" Der begriff Durchschnittpreis "), or even at p. 28 (" Das Verfahren zur Ermittelung des Geldpreises," etc.).

proportions $\frac{a'}{a}$, $\frac{a}{a}$, etc., we can hardly refuse this additional postulate.

The first four methods are all equally good if our fundamental hypothesis is strictly true. Where, as in fact, the hypothesis is only hypothetically true, the third and fourth methods, being of the nature of *means*, are apt to minimise error.

On the whole, the fourth method may appear the best; abstracting the difficulty of obtaining the proper numerical data, which is beyond the scope of this paper.

The seventh method is exposed to the objection (noticed by Dr. Lehr) that services cannot be weighed by hundredweights. Dr. Lehr's own formula is objectionable chiefly on account of its bulkiness.

It might be a good plan to take the mean of the numerical results of all the methods that are equally entitled to confidence (? the third, fourth, seventh, eighth, and—in the absence of violent price-variations—the fifth and seventh). We might thus obtain not only a better result, but also the opportunity of forming an opinion upon the *error* incident to the calculation: by how much it is likely, and by how much it is unlikely, that the result should be wide of the mark.

There are some other concrete circumstances which may entail some modifications of the general rule: (1) Unless the interval between the revisions of the units be very short indeed we must suppose that the unit is employed at times when, owing to the movements of prices (since revision), it has ceased to be exact.[1] Ideally it might be best, instead of $p'_\alpha$, $p'_\beta$, etc., present prices, to take for each article the mean of its present price and its prices in the proximate future for all the period that the unit has to function unrevised. But of course we cannot know the future prices, and therefore we must be content with taking present prices (or it may be means of the present and the immediate past) as the best representatives of the ideally preferable *mean*.* Now, considering the fluctuations of each price between two periods of revision, we see by the theory of errors that the price which fluctuates least is (*ceteris paribus*) the best representative of the mean price. And accordingly, in the combination of the different

[1] This obvious circumstance is explained at some length by Held in Conrad's *Jahrbuch* for 1871.

* It is interesting to observe that even in this section where no hypothesis securing sporadic dispersion of the prices (at any the same time) has been entertained, there still intrudes the principle of *sampling* (cp. *Journal of the Statistical Society*, 1923, p. 581, par. 2, *sub finem.*

indications of change in the value of money, there is a *prima facie* presumption that peculiar weight should be assigned to those indications which are peculiarly accurate.

But the validity of this principle turns upon very nice considerations. Where we have several measurements of one and the same thing it is indisputable that more weight attaches to the less fluctuating measures. This is true not only in the case of a real objective measurable, such as the distance between two points, but also where the *quaesitum* is a subjective mean, such as *l'homme moyen*. If, as in a case mentioned by Dr. Baxter,[1] we have two sets of measurements of heights of American citizens, the one executed with the utmost precision, the other rough-and-ready, then, in order to obtain the best value for the mean height of the American man, it would be best to affect those careless measurements with inferior weight.

But it may be otherwise when we are seeking not a single mean, but the sum of two or more. If we have to determine the distance from Dover to York *via* London, and we have very good measurements for the first distance, and very bad for the second, the best that we can do, though bad may be the best, is to add together without qualification the two means. So if we have to determine the income of a nation consisting, say, of two classes, upper and lower, for one of which the returns are very accurate, for the other very loose, still the best combination of data which is available is the simple addition of the two estimates.[2]

Yet again, if we have several estimates of such a compound mean as has been supposed, the principle of *weight* may again make its appearance. Suppose that, as Laplace proposes[3] (in the case of birth-rates), it were the practice to ascertain the statistics of "a great empire" by way of *sample*. Let observations be taken on several villages or districts, consisting each of an upper, middle, and lower class. In combining these observations so as to obtain the mean income for the empire, it would be proper to assign less weight to those localities where the returns were obtained in a more summary fashion, by a less accurate method. Further, although each estimate might not be based upon all the classes in each district, but only on a miscellaneous selection from them, still if we could divide such estimates into two classes, contrasted in respect of accuracy and differentiated

[1] United States Sanitary Commission.

[2] Supposing, of course, no wrong *animus mensurandi* or *constant error* in one direction, such as that of underrating income.

[3] *Théorie Analytique*, liv. II, *cap.*

by no other attribute, the best method of combination would be a weighted mean.

To apply these principles: (1) if, like Jevons, we content ourselves with taking *samples* of commodities rather than all commodities—a perfectly legitimate procedure, and justified alike by the theory of Laplace and the practice of statisticians, *e. g.* Jevons in his enumeration of sovereigns—then undoubtedly, the principles of *inverse probability* becoming applicable to this mode of measurement, greater weight should attach to the less fluctuating species of returns. It might indeed be a nice question how much the principle of *quantity* should be cut into by the consideration of fluctuation. Thus, if we took Mr. Giffen's [1] statistics of the variation in the prices of exports and imports as a sample (or part of one) of the change in the purchasing power of money, cotton perhaps, on account of its unique importance in respect of quantity, stands out by itself, and ought to receive full weight. But if we have several articles of about the same importance in respect of quantity but differing in fluctuation, a higher combination-weight should be assigned to the less fluctuating mass of value.

(2) A similar principle should govern our procedure, if we had to base our calculation upon returns relating not to the whole population, but only to specimens thereof. Suppose, for instance, it was sought to determine the change in the value of money in China, and that statistics could only be obtained for certain representative localities. If we make a complete enumeration of commodities we ought to take account of all articles, without regarding whether they are consumed in the same proportions or in different proportions by different persons. But if we proceed by way of sample, then we ought to assign special weight to those articles which, as Engel's law and the American labour statistics have established, are consumed in nearly equal proportions by each household throughout a large class of the community. Less weight should attach to those articles, the "sundries" of the statistics referred to, which appear more fitfully in the household budgets. How far in England we have to proceed by way of samples afforded by certain markets and certain commodities is a question not to be decided in this Memorandum. The difference upon which these distinctions turn is that which the writer, in treating of the theory of errors, has drawn between simple induction and inverse probability (see "Observations and Statistics," *Camb. Phil. Trans.*, 1885).

(3) A more obvious ground of selection is that some articles

[1] *Parl. Papers*, 1881–85.

(however large their money value) interest only a comparatively few (rich) persons. Accordingly, in constructing a standard adapted to the general requirements of the community, we ought upon utilitarian principles to treat the variations in the price of that class of articles as of comparatively little account.[1]

It may be doubted whether the practical worth of these subordinate modifications corresponds to their theoretic interest. For to assign less importance to some of the data on the ground of a deficiency of weight which is not susceptible of numerical evaluation is a practice which, though countenanced by the example of physicists in their reduction of observations, is apt to diminish confidence in sociological calculations. For the sake of a little additional accuracy it may not be worth while incurring the suspicion of cookery :—

Denique sit quidvis, simplex dumtaxat et unum.

II. We come now to the case where, the interval between the compared epochs being considerable, the quantities consumed at the two epochs are materially different, and the ratio of the quantity consumed at one epoch to the quantity consumed at the other is no longer even approximately the same for the different commodities. The difficulties presented by this case, which seemed to defy science, have been triumphed over by Professor Marshall.[2] The incommensurable proportions of the dissimilar expenditures he manages to compare by means of a series of the intercalated intermediate forms presented by the changing national inventory. Equating each term of this series to its

[1] Another modification which might be suggested is that less weight should be attached to those commodities of which the price-variations affect the general public and a particular class in different senses—a fall, for instance, benefiting the consumer, but ruining the producer. It will be found, however, a difficult and endless task to carry out this principle. For what commodities would be excepted from it? Imports perhaps, in so far as it is the foreign producer chiefly who is damaged by the fall and benefited by the rise of those prices. But with regard to the home industries, in order that the interest of the producer and the consumer should vary in opposite directions, we must suppose an equilibrium of profits to be transmitted from trade to trade, according to Ricardian principles, with a rapidity that is not supposable.

But not only is the working of the proposed principle difficult, but also it is incorrect; *here*, in this section, where our object is that the unit should afford a constant quantity of valuables to the average consumer, without reference to the number of units which the different classes of consumers have to spend. To tamper with certain items of expenditure, such as wages of Domestic Service, on the ground that these transactions belong to *distribution*, as distinguished from *exchange*, is virtually to introduce the principle of the *sliding scale*, to substitute the attribute c for C.

The exclusion of "unproductive" labour has been maintained on other grounds considered in note to p. 211.

[2] *Contemporary Review*, March 1887.

predecessor and its successor, he brings the first term into relation with the last term. Though the final and initial shapes of the *Unit* cannot even approximately be superposed, yet its content of utility is preserved constant. It is true that at each step of this process some deviation may occur. At each act of weighing something may fall out of the balance. But something may fall in also. And thus, in the absence of a constant bias towards error in one direction, there is reason to believe that—except for very long deferred payments—the result will be as accurate as that which is attainable under more favourable conditions.

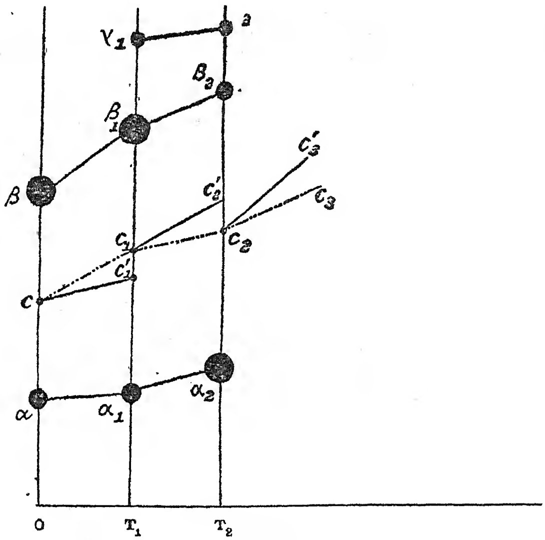


FIG. 2.

Professor Marshall's method may thus be illustrated Let us with Cournot represent ratios by logarithms, and logarithms by linear distance. But, unlike Cournot, let us take account, not only of the price, but also of the quantity of each article. Let the distance of the dot $a$ from the abscissa represent the price of the first commodity, and the size of the dot the quantity consumed (per unit of time). Let the abscissa represent time. At the initial epoch, corresponding to the origin, the purchasing power of money, the denominator of the sought unit, is represented by O$c$, where $c$ is the centre of gravity of the system initially. Now, if, during the interval O $T_1$, only money and prices were affected, other things being constant, the required (numerator of the) *Unit* would be $T_1c'_1$, where $c'_1$ is the centre of gravity of the system in its new position. But other things are not constant. There occur variations, not only in the relative

positions of the particles, but also in their masses (as shown by the varying size of the dots). Also new particles enter the system (*e. g.* $\gamma_1$ at the time $T_1$), and old ones drop out. Thus the true centre of gravity at the time, $T_1$ is not $c'_1$ but $c_1$. This point can be found at that time; but it is not available for our first edition of a tabular standard. The second edition at the time $T_2$ is similarly obtained by comparing $T_2c'_2$, the height of the apparent centre of gravity at the later epoch, with $T_1c_1$, the height of the real centre at the earlier epoch. If we join the points $cc'_1c'_2$, etc., we have the locus of apparent unit hugging the corrected curve $cc_1c_2$.

At every step there is incurred an error, say a "probable error," $\Delta u$, and accordingly what may be called an improbable error (about $4\Delta u$).[1] These errors being presumably independent, without bias in excess or defect, it follows, from the theory of errors, that the total error incurred in the course of $n$ steps is $\sqrt{n}\Delta u$. It is a nice question how frequent the revisions of the standard should be, in order that this error may be minimised. Let $\Delta t$ be that interval of time within which there cannot possibly or probably occur a change of sign in $\Delta u$, owing to a variation in those disturbances of the economic fabric which cause our standard to be inaccurate. Then it is expedient that the revisions shall take place as often as, but not oftener than, once in every such short interval. This condition points to the frequent revisals[2] contemplated by Professor Marshall.

It may be observed that Professor Marshall's solution is largely applicable to a problem kindred to ours, but which we have not supposed to be comprehended in the question set to us; namely, to measure differences in the value of money between different *places*. For instance, if the economic habits of the peoples of the Austrian empire varied by gentle gradations along a line trending from north-west to south, very much as the vital statistics of the empire are shown by Hain (in his important work on "Das Oesterreichische Reich") to vary gradually, then it might be possible, so to speak, to carry the equation of utility from Bohemia along to the Military Frontier. It is otherwise where natural and political barriers produce discontinuity; for instance, in the case of the United Kingdom compared with the United States.[3]

[1] The reader, according to his habits of thought, may regard $u$ as standing either for the sought unit or the utility which it is required to keep constant.

[2] *Contemporary Review*, March 1887.

[3] It is difficult to understand the rationale of the method by which it is proposed in the Massachusetts Labour Report for 1884 to bring together for comparison the purchasing power of wages in England and the United States.

## SECTION III.

*Determination of a Standard for Deferred Payments; not based upon the items of national consumption; calculated to afford to the consumer a constant value-in-use; no hypothesis being made as to the causes of the change in prices.* (A B C d.)

We come next to the case where the items which enter into our Unit are not copied from the statistics of national expenditure, but are selected on some other principle. Although the rule in this case is different, the ground of the rule will be found to be much the same, namely, the desirability that the advantage derived from the expenditure of a unit should be as far as possible constant. To those who admit the utilitarian character of the problem (as defined by the attributes A B C) it will appear evident that a formula other than the direct solution can only recommend itself as being a workable approximation thereto.

Among methods which may seem to have a claim to that character we may distinguish the three following :—

(1) There is first what may be called *polymetallism*, the Unit based upon the price of an aggregate of specified quantities of specified metals; and not only metals but other substances which possess an attribute ascribed to the precious metals, peculiar fixity of value.

(2) Next we place the index numbers of the *Economist*, the simple average of a number of prices, especially if, as Mr. Bourne has pointed out, care be taken to exclude the repetition of the same article in different forms.

(3) Another foundation may be afforded by a basis which Professor Nicholson (*aliud agens*, or at least not confining himself to the purpose specified in the present section) has lately laid down in the able and highly original paper which he has contributed to the March number of the *Journal of the Statistical Society*. The new basis may be described as (the value of) the "total mass of purchasable 'things,'" ("the aggregate of purchasable commodities in the widest sense" of the term). We shall sometimes, for the sake of brevity, describe Professor Nicholson's invention as the *capital* standard.

Of these secondary methods the first and second at least have some advantage in respect of convenience over the direct solution. It is quite possible that their disadvantage in respect of inaccuracy should not be very great. The error which we incur by taking some sample commodities instead of all the items of national

expenditure might be not worth correcting in view of another error with which our calculation is unavoidably affected. This is the error incident to the misfit between the consumption of the individual and that of the community. As, however, individuals resemble each other considerably in respect of consumption, there is reason to believe that this species of defect is not so important here as in the following section, where we are concerned with income derived from production.

## Section IV.

*Determination of a Standard for Deferred Payments; based upon the items of national consumption; calculated to afford to the consumer a value-in-use, varying with the national affluence, after the manner of a sliding scale; no hypothesis being made as to the causes of the change in prices.* (A B c D.)

We now abandon the idea of a fixed standard, and attempt to construct a *sliding scale*.[1] We have hitherto supposed that the average man in paying or receiving a Unit should give or take the same quantity of wealth. But is it just, is it expedient, that, when the national wealth is increasing, the creditor should demand, the debtor pay, a *constant* quantity, or quantity proportioned to the increase of general prosperity? Probably most persons would answer in favour of the former alternative.[2] But they might be embarrassed if the principle were extended to the case of declining prosperity. Would it seriously be proposed that, if money were depreciated by the decrease of goods other than money, the debtor should pay an ever-increasing amount of currency? This seems to be one of those questions of *la haute politique* which it is not our business to decide.

If it is judged desirable that the Unit should represent a quantity of wealth varying with the national affluence, a simple

[1] The idea of a *sliding scale* may not seem at first sight to be suggested by the question set to us. It will be found, however, to be implicit in much that is written on our subject by the ablest writers—those, for instance, who, in estimating the depreciation of money, dwell upon the fact that the style of living expected in each class of life, the *Lebensansprüche*, have become heightened; those, again, who, *without entertaining an hypothesis such as that which forms the definition of our section* a, still insist on including among the constituents of the Unit industrial, as distinguished from stipendiary wages, and material in addition to finished products, and exports and imports, without reference to the amount of home consumption; in fine, those who would exclude wages of domestic servants, rents, and generally *distribution* as distinguished from *exchange* on the grounds specified in note to p. 211.

[2] Cf. Poulett Scrope, *Political Economy*, (ed. 1833), p. 410.

method of effecting that condition is to put for the *Unit* the ratio of the national expenditure on articles of consumption at the later epoch to the corresponding expenditure at the earlier epoch. Employing the same notation as before, we have now the formula

$$\frac{\alpha' p'_\alpha + \beta' p'_\beta + \text{etc.}}{\alpha p_\alpha + \beta p_\beta + \text{etc.}}.$$

If it is judged desirable to compare not the absolute expenditure, but the amount relative to the number of the population, we ought to multiply the above written expression by the factor $\frac{N}{N'}$, $N$ and $N'$ representing the number of the population and the earlier and later epochs respectively.

This method appears to the writer to deserve more attention than it has received. The result would probably be much the same (in the case of short intervals at least) as for the more familiar formula. But the construction would be simpler as not requiring a mean to be taken [1] between the quantities consumed at different epochs, and the philosophic basis would be free from the difficulty which besets the equation of utility.

## Section V.

*Determination of a Standard for Deferred Payments; based upon the amount of national income or upon prices which affect the income of any class; varying with such income or prices, after the manner of a sliding scale; no hypothesis being made as to the causes of the change in prices.* (A B c d E.)

Another method of accommodating debt to the resources of the debtor is to take income as our sliding scale.[2] The received estimates of national income may be employed for this purpose. In this case the Unit might be in effect an assigned proportion of the national income per head of the population.

It should be observed that this standard, revised at most once a year, would not be adapted to the more transient fluctuations

[1] See above, pp. 212, 213.

[2] The principle of the sliding scale may be contrasted with the "Consumption standard" in two distinguishable cases—(1) First, we may suppose national wealth, the average income, to increase (or decrease) *ceteris paribus*. In this case the proper items on which the sliding scale Unit should be based appear to consist of the expenditure on finished products (our A B c D). (2) Secondly, distribution may be supposed to vary. To adjust the Unit to this variation we have to take account of wages and other distributional transactions; also of materials as affecting the incomes of certain classes

of industry. Accordingly it might be worth while to consider whether we could derive a more flexible measure of income from the prices of certain articles.* Let us begin with a simple case—an importer of articles of consumption, say of the species $\alpha$, who might be considered as paid by commission on the amount of his dealing. His income then varies with the price of $\alpha$ in the ratio $\frac{p'_\alpha}{p_\alpha}$. In the interest of this class exclusively the unit ought to be $\frac{p'}{p_\alpha}$. Or, if we suppose several such dealers, we have the weighted mean $\frac{\alpha p'_\alpha + \beta p'_\beta + \text{etc.}}{\alpha p_\alpha + \beta p_\beta + \text{etc.}}$ (assuming that the *quantities* have not materially varied between two revisions, and that the "commission" of all the dealers may be regarded as the same).

Consider next residential rent and stipendiary wages. The incomes of certain classes vary directly with these payments; yet, as these incomes are not, like the preceding, equal to a small fraction, but to the entire volume, of the transactions in question, it will not be easy to combine these data with the preceding into a properly weighted mean.

Again, when we take in ordinary wages and industrial rent, we are met by the fact that, while the income of some classes varies directly with these amounts, the interest of another class, entrepreneurs, varies *inversely*—not indeed in exact inverse ratio, but in an opposite direction to the same quantities. Again, the materials of one manufacturer are frequently the finished products of another. Accordingly the price of such articles constitutes a very bad measure of the income of all the parties concerned.

It follows from these considerations that from an examination of prices we can obtain at most a very rough and precarious indication of the variation of resources. Such a method would be related to the more exact calculation of income very much as our method A B C d was related to A B C D.

At the same time, when we consider the purpose of our sliding scale—to mitigate the evil of industrial fluctuations—it may be doubted whether this end is not realised nearly as well by a rough-and-ready method as by the most exact calculation. For a standard based upon the vicissitudes of all cannot well be adapted to the vicissitudes of each. The fit is at best so bad that it is

* It may well be doubted whether the proposed method of measuring income —which has some affinity to the method pursued in the Census of Production (190)—is better adapted than other methods to measure fluctuations of income.

not made much worse by some additional imperfections of measurement.

The character and worth of such a mean variation of price as we here desiderate might be illustrated by an imaginary example of another sort of mean, one obtained by taking the average temperature for the same day over a period of years. We have known old ladies who each year discontinued and resumed fires on the same days of the year. Suppose that they had affected even greater precision, and had burned each day a quantity of fuel based upon the mean temperature for that day averaged over a period of years. It is clear that in a climate like ours those who adopted this arrangement would some days suffer from too great heat and other days from too great cold. The arrangement would be so very defective that it would not be sensibly deteriorated by some imperfections in the method of averaging the temperatures. Suppose, for instance, that in the different years the thermometrical measurements had been effected with different degrees of completeness. For the earlier years there might be (for a given day) only sample readings of the thermometer, made two or three times a day. For the later period there might be a more continuous record of temperature. Theoretically, in combining such data more weight should be given to the more complete measurements. But practically for the purpose in view such elaboration would be nugatory.

To look at the matter more closely, let us suppose with sufficient accuracy that the income of a particular class of producers depends mainly on the prices of a certain group of articles, so that it would be convenient for that particular class that the standard for deferred payment should be regulated by the movement of those particular prices. Roughly speaking, the desideratum for that class is that the unit should be proportioned to some mean of those prices; say $\frac{mp' - \mu\pi'}{mp - \pi}$, where $p$ and $\pi$ are prices of products and agents of production respectively. But in fact the unit must be based on the prices (and quantities) of all kinds of articles. In view of the considerations touched in the text the ideally best combination of prices must be a complicated function, say of the form $\frac{F(p'_a, p'_b, \text{etc.})}{F(p_a, p_b, \text{etc.})}$. By an approximation admitted in mathematics, this expression may be written $\frac{ap'_a + bp'_b + \text{etc.}}{ap_a + bp_b + \text{etc.}}$, where the weights $a$, $b$, etc., are not (like our old friends $\alpha$, $\beta$) quantities, but coefficients deduced

from the quantities by the solution of a stupendous utilitarian problem. The varying relations between the quantities of things consumed or "used up" in manufacture, and the income of different classes—such as the importers and manufacturers of an article—all these complex correlations must be supposed duly expressed by the function $F$ and the derived simpler form. By an allowable abstraction we may suppose the course of industry so uniform that the coefficients $a$, $b$, etc., remain constant during the interval under consideration. We shall now show that for the purpose in hand—to mitigate the vicissitudes in each industry—it does not much matter what values (within wide limits) we assign to the weights $a$, $b$, etc. As announced in the Synopsis, almost any combination of the more important articles of trade is likely to be equally imperfect and equally serviceable.

Put for $p'_\alpha$, $p'_\beta$, etc., the following: $p_\alpha(1 + E_\alpha)$, $p_\beta(1 + E_\beta)$, etc. And let the displacements $E_\alpha$, $E_\beta$, etc., be made up of two portions, one affecting all articles equally, the other proper to each. Call the former $\varepsilon$, and let $E_\alpha = \varepsilon + e_\alpha$, $E_\beta = \varepsilon + e_\beta$, and so on. The unit which would be most desirable in the interest of a single class becomes of the form $1 + \varepsilon + e_\alpha$ (putting a single article as the representative of a small group). Meanwhile the general standard is of the form $1 + \varepsilon + \dfrac{ap_\alpha e_\alpha + bp_\beta e_\beta}{ap_\alpha + bp_\beta} + \text{etc.}$ The first part of both expressions coincides. But it is only by accident that the remainders can be of a piece. For *by the theory of errors* the displacement ($E_\alpha$) incident to a single article is likely to be of an order much greater than almost any mean of the proper displacements independently incident to $n$ articles. As this proposition turns upon a matter of fact, the *independence* of the proper displacements of several articles, it may be well to illustrate it by some actual statistics. In the following example (p. 227) afforded by the immense drop of prices during the crisis of 1857, $\varepsilon$, the common displacement, is considerable.

In this table the first column contains the percentage decrease for each article. The next two columns contain the differences between the average decrease (27) and the individual decreases. The modulus, or measure of fluctuation, is found to be about 16. Hence, by a well-known theorem, the probable error of the sum of $n$ differences, $n$ being large, tends to be $\sqrt{n} \times 16 \times {\cdot}477$ (a theorem which does not assume that the differences are grouped according to a known curve). Suppose, for instance, $n = 9$. The probable error of the sum of $n$ differences taken at random

PERCENTAGE DECREASE OF PRICES OF SEVERAL ARTICLES WITHIN A FORTNIGHT, NOVEMBER 1857.

(Based upon "Commercial Daily List," cited by Patterson, *Economy of Capital*, p. 191).

| | | Differences — | Differences + | Squares | |
|---|---|---|---|---|---|
| Tallow .............. | 17 | 10 | — | 100 | |
| Sugar .............. | 36 | — | 9 | 81 | |
| Cotton .............. | 14 | 13 | — | 169 | |
| Scotch pig ......... | 16 | 11 | — | 121 | |
| Saltpetre ........... | 31 | — | 4 | 16 | |
| Rice ................. | 33 | — | 6 | 36 | |
| Silk ................. | 33 | — | 6 | 36 | |
| Linseed ........... | 17 | 10 | — | 100 | |
| Linseed oil ......... | 20 | 7 | — | 49 | |
| Tin ................. | 10 | 17 | — | 289 | |
| Tea ................. | 25 | 2 | — | 4 | |
| Pimento ........... | 40 | — | 13 | 169 | |
| Turmeric ........... | 50 | — | 23 | 529 | |
| Shellac .............. | 33 | — | 6 | 36 | |
| Jute ................. | 40 | — | 13 | 169 | |
| Hemp .............. | 16 | 11 | — | 121 | |
| Sums ......... | 431 | 79 | 80 | 2,025 | |
| Means ......... | 27 | 10 | 10 | 127 | = Mean square of error |
| | | | | 254 | = Modulus squared |

should be about 23. This may be illustrated by actually taking some batches of nine, say the first nine, tallow to linseed oil, the last nine, linseed to hemp, and a central nine. The sum of the first set of differences is $-51 + 25 = -26$. The sum of the second set of differences is $-47 + 55 = 8$. The sum of a third set, from Scotch pig to pimento, is $-58 + 29 = -29$; while if we put out the Scotch pig and take in turmeric we obtain $+5$. These observed results are very consonant with the theory that the probable error is 23. Hence the probable error of the *mean* of nine differences is $2\frac{5}{9}$. Meanwhile the probable error of *any single* difference may be found by observing that the "quartiles," in Mr. Galton's phrase, occur on the one side between $-10$ and $-11$, and on the other side between $+6$ and $+9$, giving a probable error of, say, 9. Or we may proceed more hypothetically, and, assuming that the grouping (of the differences) is conformable to the [1] normal type, find the probable error ($\cdot 477 \times$ modulus) about 8. Thus the displacement of the single article is seen to exceed the mean displacement of several articles in about the degree required by theory.

We have taken the simple (arithmetical) mean. But much the same would be true if we had taken *any* weighted mean * of all prices, in particular the ideally best, whose weights are $ap_a$,

[1] The probability-curve [normal law of error].

* Within limits; as shown in the Second Memorandum.

$bp_\beta$, etc. (provided at least those coefficients are not extremely unequal). The deviation of the particular standard from the general standard is apt to be so considerable that it does not much matter by what system of weights we determine the general standard. The unit best in the indivdual interest is as we have seen above (p. 226), $1 + \epsilon + \epsilon_\alpha$. The unit in the general interest is of the form $1 + \epsilon + \dfrac{A\epsilon_\alpha + B\epsilon_\beta + \text{etc.}}{A + B + \text{etc.}}$ (putting $A = ap_\alpha$, and similarly $B$). The deviation of the former from the latter is of the form $\epsilon_\alpha - \dfrac{Ae_\alpha + Be_\beta + \text{etc.}}{A + B + \text{etc.}}$. Now, if $e_\alpha$, $e_\beta$, etc., be on an average of the order $e$, then by the theory of errors their weighted mean, the latter part of the expression just written, will be of the order $e \dfrac{\sqrt{A^2 + B^2 + \text{etc.}}}{(A + B + C + \text{etc.})}$, an expression which tends to zero as the number of the coefficients is increased. The unavoidable discrepancy between the particular and general interest is therefore not likely to be much diminished by a more exact calculation of weights when those weights are numerous.—Q.E.D.

Take, for example, the statistics above cited, where there are only sixteen items, and let us suppose the weights so disparate as the cardinal numbers 1, 2, . . . 16. If we based our unit on the simple arithmetic mean, we have $\epsilon = \cdot 27$, and for the *Unit* 1·27. Now this *Unit*, as applied to each particular interest, is apt to be out by about ·1, or 10 per cent. In the tallow interest, for instance, 1·17 would have been the best unit; if we legislated exclusively in the sugar interest the unit would be 1·36. Let us see now how these misfits would have been mended by a more elaborate adjustment of the standard. The expression $\dfrac{\sqrt{A^2 + B^2 + \text{etc.}}}{A + B + \text{etc.}}$ becomes when $A = 1$, $B = 2$, etc., about ·3. The correction then upon the arithmetic mean ·27 would be of the order $\cdot 3 \times \cdot 1$ ($e$ being of the order ·1),[1] that is, ·03, or 3 per cent. This theorem may be verified by actually assigning the weights 1, 2, 3, etc., to the percentages above cited. The weighted mean $\dfrac{1 \times 17 + 2 \times 36 + 3 \times 14 + \text{etc.} + 16 \times 16}{1 + 2 + 3 + \text{etc.} + 16} = 28 \cdot 8$. If we reverse the order of importance, and, beginning at the bottom of the list, assign a weight 1 to hemp, 2 to jute, 3 to shellac, etc., we obtain for the weighted mean 25·6. The difference in each

[1] Assuming that each of the articles (tallow, sugar, etc.) is subject to the same law of fluctuation, we may conclude (from an examination of the table) that the average error for any article is 10 per cent.

case between the simple and weighted mean is even less than theory predicts. Suppose the corrected unit becomes 1·25, the tallow interest will now be out by *eight* per cent. instead of *ten* per cent. from the standard best for them exclusively—no very great gain, and partly (by hypothesis of course, not wholly) [1] balanced by the loss of the sugar interest, who are now more out than before. *A fortiori* when the number of articles is greater than *sixteen*.

The general conclusion is that for the purpose in hand it is not much matter what sort of mean we take; provided that the weights assigned to the different articles are not very unequal, and provided that there is no reason to think that the ideally best system of weights would be very unequal. The test that factors $A$, $B$, etc., are not sensibly unequal is the condition that $\sqrt{A^2 + B^2 + \text{etc.}} \div (A + B + \text{etc.})$ should be small; which is true enough within very wide limits (*e. g.* in the case of sixteen weights being respectively 1, 2, 3, etc., 16). When there are a few relatively very large interests, such as possibly in England cotton, iron, and ordinary wages, then in constructing our general sliding-scale we should pay special attention to those interests; though from the considerations mentioned above (p. 204) we are not entitled to assume that the weight to be attached to (the price-variation for) each interest is *directly proportioned* to the magnitude of the transactions.

It will be observed that this reasoning turns upon the unique interest of particular groups of persons in the prices of particular articles, on the circumstance of *division of labour*.[2] The conclusion as to the worth of our result is therefore not equally applicable to what may be called the *consumption* (A B C) as distinguished from the *production* (A B c) standard. For the rest the latter calculation resembles the former in being amenable to similar secondary modifications (see above, p. 217). For instance, upon the third of the principles referred to a variation of wages ought to affect the *Unit* more than an equal variation of profits as concerning a greater number of persons.

[1] If we suppose the *weights* 1, 2, . . . 16 to constitute the ideally best system, that which affords the maximum sum total of advantage to all.

[2] Compare the remarks of Von Jacob cited by Mr. Horton in his admirable chapter on the *Standard of Desiderata; Silver and Gold,* p. 39

## SECTION VI.

*Determination of a Standard for Deferred Payments ; based upon the amount of national capital ; varying with such amount, after the manner of a sliding scale ; no hypothesis being made as to the causes of the change in prices.* (A B c d e.)

The next category is distinguished by the condition that the basis of the required sliding scale is capital rather than income. This Unit might be specially adapted to certain debts; for instance, in estimating the capital (but not the interest) of sums raised upon mortgage of fixed capital. It is interesting to inquire what sort of weight should be assigned to wages for the purpose here defined. May we measure the importance of wages as a means for paying off capital by the lump sum which the wage-earner is able to raise upon the prospect of his earnings by way of insurance ?

With reference to this most important application of Professor Nicholson's method, it may be proper here to introduce a remark which is applicable also to other uses of that method. When its originator is met with the difficulty that articles do not increase uniformly, he argues that " the change in the purchasing power of the standard is found by dividing the value of the new inventory at the old prices by its value at the new." And he is understood to regard this method as preferable to the converse method, dividing the value of the *old* inventory at the old prices by its value at the new. His reasoning turns upon the postulate, " Let the total value of the new inventory (consisting of different quantities of the old items) reckoned at the old prices be $v_1$ and the total value of the old inventory, also at old prices, be $w_1$; then $\frac{v_1}{w_1}$ is the measure of the increase in the quantity of wealth." In this passage read for " old prices " *new prices*, for $v_1$ read $w_2$, and for $w_1$ a new symbol $v_2$, and you will have a postulate no less true, or no more arbitrary. According to the substituted principle, " the measure of the increase in the quantity of wealth " is $\frac{w_2}{v_2}$; which being multiplied by $\frac{w_1}{w_2}$, by parity of reasoning with that employed by the author on the page referred to, gives for the " measure of the new purchasing power compared with the old " $\frac{w_1}{w_2} \times \frac{w_2}{v_2} = \frac{w_1}{v_2}$; which being interpreted means dividing the *old* inventory at the old prices by the value of the same inventory at the new prices.

Observing that the "change in the purchasing power of the standard" is the reciprocal of what we have elsewhere called the Unit, we see that the two methods just reached correspond to the formulæ (2) and (1) of our section A B C D (above, p. 212). It is important to point out that neither of these solutions is afore nor after the other.[1] Otherwise there might be an objection to the use of a symmetrical mean between the two, such as has been recommended.

## Section VII

*Definition of the Appreciation [or Depreciation] which it is the object of Bimetallism and similar projects to correct; no hypothesis being made as to the causes of the change in prices.*

The variation in the value of money which we have been hitherto considering is that which is corrigible by the adoption of a "Unit" for deferred payments. For different purposes different formulæ are appropriate. The purpose next in importance to the construction of a Unit (if not indeed, as some think, prior in importance and the main scope of the task set to us) is to correct the instability of trade, to restore the level of prices by augmenting the quantity of legal-tender currency, whether by Bimetallism or the increase [2] of paper-money.

Now, if we might assume all prices diminished uniformly, like the shadows of objects as the sun advances from the east, the problem would be very simple. It is an intelligible proposition that the *status quo* might be restored by an elevation of the objects all round. And the significance of the proposition need not be impaired if we suppose the objects waving and oscillating, and some of them depressed, others elevated in random fashion between the two epochs at which the shadow-lengths are observed. But we are not entitled *here* to make an assumption, which is the characteristic of the following section. We must rather seek a rule available even in the case in which one large category of objects may be considerably and uniformly elevated, another depressed;

[1] The question whether it is easier to get present quantities at old prices than old quantities at new prices does not come within the scope of this memorandum.

[2] *E. g.* By introducing £1 notes in England, or according to some more daring plan, such as those proposed by Professor Marshall (*Contemporary Review* March 1887, note near end), Faucher (*Jahrbuch für Gesetzgebung*, 1868), and others.

where the variations do not present any true mean or normal type. Our formula should be irrespective of such an hypothesis here equally as in the previous sections.*

The operation of augmenting the currency proper to the present section, as contrasted with the method of making contracts in Units, presents the following four distinguishing characteristics :—

(1) The infusion of money is not adapted to correct the more transient fluctuations of prices due to the oscillations of credit; whereas our Producers' Unit—including commodities other than finished products—is specially adapted to the correction of transient fluctuations.**

(2) The operation of the proposed remedy requires time. The detection of the evil—the secular as distinguished from the tidal variation of price-level—also requires time. It follows that the epochs which are to be compared in respect of purchasing power are separated by a considerable interval. Hence the calculation of a Unit to express change in the purchasing power of money must be of the less exact sort, which might be distinguished as *integral*.

(3) Again, the area which is affected by the augmentation of currency is very extensive, at least when (as in the case of Bimetallism) the added circulation consists of precious metal. Accordingly, the appreciation which is to be corrected by that remedy must relate to a very wide area, the whole system of states in monetary communication; that is, the greater part of the civilised and uncivilised world. Now, the larger and more diversified the public to which there is applied any regulation based upon the mean requirements of the average man, the less perfectly is that type or norm likely to be adapted to the requirements of the individual. The correction of appreciation, which may be effected by the infusion of metallic money, is therefore likely to be of less benefit than that which attends the method of contracting in Units.

(4) Moreover, in the latter case the measure of the evil and of the remedy is the same. The same calculation which gives the appreciation assigns the Unit in terms of which debts are to be paid. But it is not so where the remedy is the augmentation of legal-tender money. The extent of the evil (the appreciation) having been found, the extent of the remedy is still to seek.

* Here, and in the context, there are omitted sentences identifying the desired ormula with conceptions already defined.

** But see note added to Section V above, p. 224.

For it is a very naïve [1] conception that, in order to increase prices all round in a certain ratio, it is necessary and sufficient to increase the quantity of legal-tender money in that ratio.

These imperfections of the method under consideration may be thus summed up: (1) It cannot even aim at certain objects which are within the range of the alternative method. (2) The objects which it does aim at are not sighted so clearly; its shots are apt to be very wide of the mark. (3) The advantage of hitting the mark, the prize to be won, the quarry to be brought down, is not so considerable as in the case of the alternative method. (4) Lastly, in the one case we shoot point-blank; having discovered the position of the object, we have the direction in which we ought to aim. But in the other case the trajectory has yet to be calculated, in virtue of which, being given the position of the object, we can deduce the direction of our aim.*

## Section VIII.

*Determination of an Index irrespective of the quantities of commodities; upon the hypothesis that there is a numerous group of articles whose prices vary after the manner of a perfect market, with changes affecting the supply of money.* (a F.)

So far we have made no supposition as to the cause of the phenomenon which is under measurement. As far as we have been concerned there might have been a number of heterogeneous causes, or, what is even more unfavourable to calculation, a few great causes; as if one large class of prices were heightened according to the law of diminishing returns, while other prices, also forming a large class, were lowered by increased division of labour, and others by improved means of transport. We are now to entertain an hypothesis, namely, that there is an effect capable of being discovered and worth discovering, due to [2] "causes which operate upon all goods whatever," or at least upon a considerable group of goods; for instance, the increased

[1] See below, Section IX.

* There is here omitted as not particularly appropriate here an elaborate metaphor designed to illustrate the significance of an hypothesis implying the presence of a true mean or good average. Suffice it to repeat "if one great group of commodities varies pretty uniformly in one direction, and another in a different direction (or even in the same direction, but in a markedly different degree), then the task of restoring the level of prices can no longer be regarded as a purely objective *quæsitum*, a currency problem.

[2] Mill, *Political Economy*, Book. III. chap. viii. § 2.

quantity, or efficiency, of legal-tender money, or the improvement of money-saving expedients.[1]

The simplest hypothesis of this sort is the proposition in the text-books that prices vary inversely with the quantity of money, other things being equal. But we are not restricted to the "Quantity Theory."[2] It is sufficient for our purpose that there should be a circle of commodities, including money, such that the equilibrium of exchange between them should continually be readjusted by a comparatively frictionless play of market-forces. That this condition does hold approximately with respect to a large group of articles is shown in the case of Austria by Dr. Kraemer in his important work on Austrian Paper-money. From the statistics given in his Chapter III. there can be no doubt that a change in the "valuta" of currency does enter into, and might be extricated from, the prices of a certain set of commodities. The following articles may be instanced as particularly sensitive:—*Wool*, *spirits*, *rape-seed*, *undressed leather*, and, in general, articles of foreign trade. These observations are supported by the copious statistics adduced by Herska, Bela Földes, and others. The only question is whether we ought not to regard all commodities, rather than only some commodities, as varying with the *agio*. No doubt it is a delicate question, and only to be decided by the proper mathematical methods of statistics, whether it is possible to extricate a mean variation in the value of money from the changes of particular prices. It seems to be so in the case of Austria. In the case of the United States, if we could accept the law laid down by Mr. Delmar as to the propagation of a change in price, we could not hope for a sufficiently large group to afford a real average. But the statistics adduced by Hock, in his history of the finance of the United States, show conclusively that in correspondence with the condition of the inconvertible currency and the state of credit there did extend pretty uniform waves of disturbance over a part, if not indeed the whole, of American industry.

The proposition which has been proved for inconvertible currency is shown to be true for metallic money—as regards, at least, a certain zone of industry—by the index numbers of the *Economist*, the statistics adduced by Soetbeer, Laspeyres, and others.

[1] A good enumeration of causes that are apt to cause a general variation of prices in the case of Inconvertible currency is made by Bela Földes in the *Jahrbuecher für Natl. Oekonomie*, 1882.

[2] The discussions in Section IX will show how far the writer is from regarding this theory as generally applicable.

Assuming, then, that there is, or may be, over a certain region of the industrial world a mean disturbance of the sort described, it would be a significant operation to take the average of all the relative prices, *irrespective of the quantities of the corresponding commodities.* We should thus obtain a mean elevation or depression which may be described as a figure such that, if we took any ware at random, that figure [1] would be more likely than any other to be equal to the relative price of the selected ware. A similar typical mean of human heights (irrespective of other attributes) has proved a useful implement of statistical induction in the hands of Mr. Galton, Dr. Charles Roberts, and others.

A more exact illustration is afforded by the following physical analogies. Suppose it were required to measure the force of gravitation in the neighbourhood of a mountain. Our data might consist of a set of pendulums, all disturbed from the vertical by the attraction of the mountain, and each further subject to proper disturbances. The displacement from the vertical constituting the required measurement might be found by taking a mean of the displacements suffered by all the pendulums. Now, *from what we know of the action of gravity*, there is no reason to think that the displacement of a larger mass gives in general a better measure of the common disturbing agency, the gravitation force, than a smaller mass does. Hence, in taking the mean of the displacements, there is no propriety in assigning more importance to the displacement of the more massive pendulum. If we do assign preferential importance, it should be on other grounds, namely, that the proper disturbances of some pendulums are apt to be less serious than those of others. The *combination weights* (or "multiplier weights," in Sir G. Airy's phrase) determined by such considerations must be carefully distinguished from the "weight" in the ordinary sense. The pendulum weightiest in the former sense might be lightest in the latter sense. Another caution is to distinguish the present investigation from that whose object is the displacement of the *centre of gravity* of the system,[2] a *quæsitum* which does not presuppose any common disturbing agency.

Again the problem special to this section has been likened to the problem of discovering the proper motion of the solar system by means of the apparent movements of the stars. Let us

[1] In short, the greatest ordinate of the curve of price-variations.

[2] Analogous to the calculation of Units in our earlier unhypothetical sections.

suppose, for the sake of illustration, that the *line* in which the solar system moves has been ascertained. The only questions are in which direction of that line, positive or negative, say towards or from a certain star in Hercules, and at what rate, we are moving; how far we have moved between two given epochs. Now, if we take several groups of stars at random, say (as in fact is done) some groups in the northern hemisphere, and others in the southern, and for each of these groups we take the mean of the apparent motion of the stars along the given line; then, if the mean resultant is much the same [1] for every group, we may be reasonably certain that the phenomenon is due to a common cause, which is doubtless no other than the proper motion of the solar system. Suppose, however, that the motions of the stars did not conform to what may be called a true mean. Suppose that what Mr. Proctor calls "star-drift" was prevalent on a much greater scale than he has found to be the case; that the Milky Way, together with other zones, moved off *en bloc* in one direction, while the Great Bear carried off another half of the heavenly host in the opposite direction. In this case we should no longer be able to detect the motion proper to the solar system. The peculiar grip which a plurality of independent events affords to the calculus of probabilities now becomes wanting.

It is to be observed that, in assigning importance to the different indications given by the apparent motions, the criterion is not the *mass* of the star, but its "weight" [2] in the sense of affording a better measure of the *quæsitum*, the motion of the solar system.

Similarly, in the problem before us it must be either given by previous experience (as in the case of our first illustration), or discoverable from the data themselves (as in our second illustration), that there is a *true mean;* that one set of commodities, such as the products of extractive labour, has not risen *en bloc*, while another set, as manufactures, has fallen. Without that condition we cannot follow Jevons in reasoning by the principles of probabilities that gold has been depreciated (or appreciated) to a certain extent. With that condition we may follow Jevons in taking a mean of price-variations, *irrespective of the quantities of the commodities*.

The problem before us may be thus defined. Given a number

[1] If it be asked what extent of difference between the means of different groups is to be expected and may be regarded as insignificant, the answer is supplied by the mathematical Theory of Errors. See the writer's paper on *Methods of Statistics*.

[2] Depending on considerations not here relevant.

of observations consisting each of the ratio between the new price and the old price of an article, to find the mean of these observations—the objective or quasi-objective mean—as distinguished from those combinations in the preceding sections which were prescribed by considerations of utility. The problem as thus conceived belongs to that higher branch of the calculus of probabilities which may be called the doctrine of errors. Upon the theory of errors are based two kinds of problem; of which the first is exemplified by the method of determining the true position of a star from a number of separately erroneous observations, the second, by the method of constructing the typical stature of a people, *l'homme moyen*, from the measurement of a great number of individuals. To which of these analogies—the more, or the less, "objective" species of mean—our case most corresponds is a nice inquiry, varying with the shades of hypothesis.[1] Upon either view the practical rules for extricating the mean are much the same. They may be arranged under two headings, relating (1) to the form in which the given observations are to be combined; and (2) the relative importance to be assigned to the different observations.

(1) As to the first point the general rule is that in the absence of special presumptions to the contrary an arithmetical mean (or linear function) of the given measurements is the proper combination.[2] That is to say, if the different measurements are $r_1$, $r_2$, etc., each purporting to represent one and the same object, in our case the appreciation or depreciation of money, the proper combination of these data is—

$$\frac{w_1 r_1 + w_2 r_2 + \text{etc.}}{w_1 + w_2 + \text{etc.}};$$

where the factors $w_1$, $w_2$, etc., are *weights*, such that if $w_1$ is greater than $w_2$ then $r_1$ contributes more to the result than $r_2$.

This general presumption in favour of the arithmetic mean may, however, be rebutted by specific evidence in favour of some other mean, and it is here submitted that in the case of prices there does exist such specific evidence in favour of the *geometric mean*.

It appears that prices group themselves about a mean, not

[1] Consider the illustrations given below at p. 247.

[2] The ground of this presumption is partly that the arithmetic mean is one of the *simplest* methods of combination; partly that it is specially adapted to a species of observation which is very extensive *in rerum naturâ*, which may be said to be always tending to be realised, the exponential law of error, or probability-curve.

according to a symmetrical curve like that which corresponds to [1] the arithmetic mean, but according to an unsymmetrical curve like [2] that which corresponds to the geometric mean. Before adducing the empirical proof of this proposition it may be well to consider what *a priori* grounds we might have for preferring the geometric mean. There are [3] those who consider that the mere accumulation of agreeing experiences can seldom suffice, without some antecedent probability, to establish an inductive conclusion.

It has been shown by Mr. Galton and others that the geometric mean is adapted to a particular species [4] of observations, which may be described as *estimates*. For instance, the estimates which different persons (or the same person at different times) might make of a certain weight would be likely to err more in excess than in defect of the true objective weight, and in such wise as to render the geometric mean of such a series of estimates the proper method of reduction. This law of prizing may well extend to prices. The fluctuating estimates which from time to time a person might make of the [5] utility of an object, as measured by the quantity of some other object, *e. g.* money, might well fluctuate according to the law which has affinities to the geometric mean. So far then as changes in price might depend upon fluctuations in demand,[6] there is something to be said in favour of our proposition.

Again, there exists a simple reason why prices are apt to deviate much more in excess than in defect: [7] namely, that a price may rise to any amount, but cannot sink below zero.[8]

Lastly, the supposed tacit combination which everywhere

[1] The probability-curve [normal law of error].

[2] The curve described by Dr. Macalister in his paper on *The Law of the Geometric Mean* in the *Philosophical Transactions*, 1879.

[3] G. C. Lewis as quoted by Dr. Bain in his *Logic*.

[4] Wherever the law of Fechner applies. See papers by Mr. Galton and Dr. Macalister, *Proc. Royal Soc.* 1879.

[5] *I. e.* the "final utility."

[6] Variations in what is technically called the demand-curve.

[7] As in the annexed diagram.

[8] That price should be, in Dr. Venn's phrase, a "one-ended phenomenon" may raise a presumption in favour of an asymmetrical grouping, but by no means dispenses with empirical verification. For the same presumption exists not only in the case of many anthropometrical and other statistics which prove to be symmetrical, but also in cases where there is an asymmetry in the sense contrary to the theory, an extension of the *lower* limits of the representative curve. Such are the statistics of barometrical height arranged by Dr. Venn in *Nature*, Sept. 1, 1887; the statistics of eyesight given by Dr. Charles Roberts in the *Medical Times*, Feb. 1885; the grouping of Italian recruits by Signor Perozzo in *Annali di Statistica*, 1878.

exists between dealers may prevent prices falling as low as from time to time they otherwise would according to the play of supply and demand.

There is therefore at any rate no *a priori* presumption against the proposition that price-returns are apt to group themselves in an unsymmetrical curve of which the range in excess is greater than in defect. In favour of this proposition the following empirical evidence is adduced :—

In the first table are examined the prices of twelve commodities during the two periods 1782–1820, 1820–1865. The maximum and minimum entry for each series having been noted, it is found that the number of entries above the "middle point," half-way between the maximum and minimum, is in every instance less, and in some instances very much less, than half the total number of entries in the series. In the twenty-four trials there is only one exception to the rule, and in very few

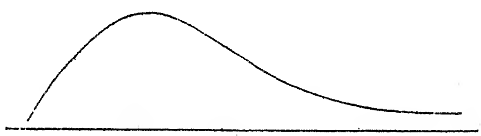

FIG. 3.

cases even an approach to an exception. We may presume then that the curves are of the lopsided character indicated by the accompanying diagram. For the "median" [or point having as many entries above as below it], which upon the supposition of symmetry ought to be about coincident with the "middle point" as above defined, or at any rate as often above as below it—this median is in every instance but one (fodder, 1798–1820) below the middle point.

Fig. 3 very well represents the prices of corn during the periods 1261–1400, and 1401–1540 given in Professor Rogers' *History of Agriculture*. The abscissa in the figure represents prices, and the ordinate the number of years in which the corresponding price was enjoyed. It will be found that in both cases the maximum elevation, the greatest ordinate of the curve, occurs between five and six (shillings). Below that maximum-point, in both cases the curve does not sink more than two or three shillings (2*s*. 10¾*d*. is the lowest entry), while above that point one curve stretches out to 14, the other to 16. There can be no doubt about the fulfilment of an unsymmetrical law. Further verification of the law may thus be obtained from the

earlier series of statistics. Compare the decennial averages (of corn prices) given by Professor Rogers with the annual returns on which they are based. The " middle point," half-way between the maximum and minimum of each decade, is in almost every case above the average. There are only three exceptions out of the fourteen decades, viz., 1271–1281, 1281–1291, 1371–1381; and one of these exceptions is not an instance to the contrary, the middle point exactly coinciding with the average.

If the prices are similarly examined by decades for linen (Vol. I. p. 593), clouts, and other commodities, it will be found that the rule holds, with no exceptions, or trifling ones. Thus for clouts there is not a single exception during twelve decades, 1271–1390. The only exception which Professor Rogers' statistics show is the decade 1391–1400.

Similar results are presented by the table of price fluctuations in the *Massachusetts Labour Report*, 1885, p. 459. Out of seventy-eight commodities nine only have the minimum further below the average than the maximum is above it. And those exceptions are slight in respect of extent, while the exemplifications are often marked.

Examination of Variation of Prices, 1782–1865.

(See Jevons, *Currency and Finance*, Table VIII. p. 144.)

| | | Minimum. | Maximum. | Middle point between Max. and Min. | No. of returns above middle. | Median. |
|---|---|---|---|---|---|---|
| Oriental products | 1782–1820 | 65 | 107 | 86 | 15 | 84 |
| | 1821–1865 | 30 | 80 | 55 | 11 | 45 |
| Tropical food | 1782–1820 | 60 | 102 | 81 | 8 | 65 |
| | 1821–1865 | 34 | 65 | 49½ | 14 | 48 |
| Metals | 1782–1820 | 89 | 169 | 129 | 12 | 113 |
| | 1821–1865 | 71 | 123 | 97 | 13 | 87 |
| Iron | 1782–1820 | 67 | 139 | 103 | 16 | 99 |
| | 1821–1865 | 35 | 114 | 74½ | 10 | 55 |
| Timber | 1782–1820 | 54 | 533 | 293½ | 4 | 116 |
| | 1821–1865 | 62 | 137 | 99½ | 9 | 90 |
| Oils | 1782–1820 | 81 | 166 | 123½ | 14 | 105 |
| | 1821–1865 | 75 | 121 | 98 | 10 | 90 |
| Dye materials | 1782–1820 | 64 | 157 | 110½ | 10 | 98 |
| | 1821–1865 | 30 | 98 | 63 | 7 | 36 |
| Fibres, cotton, wool, etc. | 1782–1820 | 88 | 214 | 151 | 4 | 130 |
| | 1821–1860 | 61 | 121 | 86½ | 17 | 78 |
| | 1821–1865 | 61 | 129 | 95 | 12 | 88 |
| Cotton | 1782–1820 | 57 | 204 | 130½ | 2 | 87 |
| | 1821–1860 | 21 | 63 | 42 | 9 | 33 |
| | 1821–1865 | 21 | 128 | 74½ | 4 | 36 |
| Corn | 1782–1820 | 99 | 252 | 175½ | 9 | 134 |
| | 1821–1865 | 92 | 176 | 134 | 18 | 128 |
| Wheat | 1782–1820 | 81 | 231 | 156 | 12 | 131 |
| | 1821–1865 | 78 | 151 | 114½ | 18 | 113 |
| Fodder | 1798*–1820 | 118 | 308 | 213 | 12 (out of 23) | 214 |
| | 1821–1865 | 156 | 250 | 203 | 20 | 199 |

* Return previous to 1798 wanting.

The next statistics present not time fluctuations, but place fluctuations. In the *Illinois Statistics of Labour Report*, vol. iii. p. 340, are given the prices of thirty-eight articles in 34 different [1] towns. Examining the series of prices for each article, we find that there is fulfilled in almost every case the law that the maximum is further from the average than the minimum is. Most of the exceptions are very slight, and disappear if we take in the penultimate the observations *penemaximum* and *pene-minimum*. The only real exceptions are mackerel, fresh fish, cheese, butter, and crackers, five articles out of thirty-eight. The odds against such a phenomenon occurring by accident are hundreds of thousands to one.

Lastly, let us take price returns for the same time and locality, but for different articles.

This table is extracted from Jevons' table of Proportional Variation of Prices, *Currency and Finance*, p. 144. The "median" is the point which has as many observations above as below it. Where, as in the majority of the rows above, the number of entries is even twelve, namely 12, the point half-way between the sixth and seventh has been taken as the median. The sixth and seventh being in almost every case close together, there is very little of arbitrariness in this procedure. The fact that the maximum is in every case further from the median than the minimum shows the lopsided character of the price-curves. The median has been used instead of the arithmetic mean only for convenience of calculation. Much the same conclusions would evidently have followed from the use of the arithmetic mean,

| | Oriental Products. | Tropical Food. | Metals. | Iron. | Timber. | Oils. | Dyes. | Fibres. | Cotton. | Corn. | Wheat. | Fodder. | Max. | Min. | Med. | Distance from Med. to Max. compared with Distance from Min. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1783 | 101 | 87 | 100 | 97 | 108 | 94 | 92 | 112 | 102 | 127 | 110 | — | 127 | 87 | 102 | + |
| 1791 | 89 | 72 | 100 | 92 | 85 | 82 | 77 | 96 | 64 | 112 | 99 | — | 112 | 64 | 85 | + |
| 1801 | 80 | 73 | 139 | 139 | 167 | 134 | 108 | 142 | 114 | 232 | 222 | 244 | 244 | 73 | 139 | + |
| 1811 | 74 | 60 | 148 | 106 | 381 | 136 | 107 | 149 | 66 | 167 | 178 | 308 | 381 | 60 | 148·5 | + |
| 1821 | 68 | 63 | 101 | 82 | 116 | 89 | 74 | 112 | 53 | 116 | 114 | 182 | 182 | 53 | 113 | + |
| 1831 | 49 | 48 | 80 | 63 | 104 | 90 | 49 | 87 | 33 | 150 | 135 | 199 | 199 | 33 | 88·5 | + |
| 1841 | 51 | 53 | 90 | 61 | 113 | 97 | 40 | 88 | 37 | 140 | 131 | 227 | 227 | 40 | 89 | + |
| 1851 | 36 | 41 | 73 | 36 | 68 | 90 | 31 | 77 | 27 | 98 | 78 | 163 | 163 | 27 | 77·5 | + |
| 1861 | 36 | 38 | 88 | 37 | 69 | 111 | 32 | 96 | 39 | 135 | 113 | 213 | 213 | 32 | 78·5 | + |

[1] For some few of the towns more than one price is quoted.

as the writer has verified for the years 1801, 1821, 1831, 1851. The figures in each row overlined and underlined respectively are the penemaximum and peneminimum. If we compare the distances between each of these and the median the series of signs is found to become $0 + + + - + + + +$. The exceptional year is 1821. If we examine the arithmetic mean for that year the exception still exists, but in a less marked degree.

Such a curve is well represented by the equation

$$y = \frac{h}{x\sqrt{\pi}} e^{-h^2(\log x)^2},$$

where $h$ is a constant corresponding to the dispersion, or *écart*, of the curve (see Dr. Macalister's paper "On the Law of the Geometric Mean" *Proc. Roy. Soc.*, 1879), and compare the present writer's "Observations and Statistics" (*Cam. Phil. Trans.*, p. 149). Hence, given a number of observations deviating from the mean about which they are grouped, each according to a law of the general form above stated, the most probable value of the mean deduced from these observations will be the *weighted geometric mean* give by the equation

$$\log x = \frac{h_1 \log x_1 + h_2 \log x_2 + \text{etc. } h_n \log x_n}{h_1 + h_2 + \text{etc.} + h},$$

where $x$ is the sought mean, $x_1$, $x_2$, etc., are the given observations, and $h_1$, $h_2$, etc., are the weights, of which more hereafter.

It must be remembered, however, that there may be other means adapted to represent the bias which has been observed, in particular what may be called the unsymmetrical probability-curve, elsewhere described by the present writer (*Lond. Phil. Mag.*, April 1886). Nor, again, is it to be supposed that *all* statistics of prices are grouped unsymmetrically. Where the entries are average prices based on a great number of items it is agreeable both to [1] theory and the writer's observations that the normal symmetrical "probability"-curve will set in. It will be found difficult, for instance, to trace evidence of lopsidedness in the five-year averages given by Soetbeer.[2]

The evidence adduced appears to afford a reasonable presumption that the required method of combination is some form other than the arithmetic mean, of the general character of the geometric mean. Those who have followed Jevons' investigations will be

[1] See in *Methods of Statistics* the statement of the proposition that the average of a *large* number of returns obeying individually *any* law of grouping tends to conform to the probability-curve.

[2] *Materialen*, pp. 99–114.

familiar with the proposal that the logarithm of the required mean or general percentage should be equated to the arithmetic mean of the logarithms of the percentages special to each article. To which it is now to be added that this arithmetic mean need not be *simple*, but may be *weighted* in the sense above indicated (p. 242); *e. g.*—

$$\log x = \frac{w_1 \log x_1 + w_2 \log x_2 + \text{etc.}}{w_1 + w_2 + \text{etc.}}$$

What then are these weights to be? is our *second* inquiry.

(2) The theory of errors supplies the following rules—of which the first two have been already implied in our statement of the problem—($\alpha$) In the first place no weight should be attached to a class of observations known to be affected with what is called a *constant error*, or uniform bias in one direction. It is supposed of course that only the fact, but not the amount, of the error is known; otherwise it would be possible to get rid of it. In our case this rule dictates to reject all prices which are not amenable to that play of a perfect market whose change of level we have to investigate. The writer is far from pretending that this region of permeability can at present be marked off with precision. However, a rough delimitation may be effected by researches like Dr. Kraemer's.

Assuming then that we have selected a set of percentages which may be regarded as accidental deviations from a common mean, on what principle should more importance be attached to one indication of change rather than another? The second ($\beta$) maxim which we have to apply is that the observations should be independent. This condition excludes the prices of the same commodity at different stages of production, since these prices are closely interdependent. Or, if we must take account that at each stage some fresh cause of fluctuation—source of "error" —is introduced, at any rate each price-return is not to count for one, but only for a fraction.

Here arises the question whether a commodity extensively consumed like meat or cotton ought not to count for more, in so far as its price is a mean of a greater number of transactions, than Cloves and Peppper. The answer is that those transactions are not *independent*. The law that there can be only one price in a market *prima facie* removes the presumption in favour of the more largely consumed commodity. There is no analogy between the average price of such a commodity and a mean founded upon a specially large number of independent observations in theory

at least, and for the purpose of a first approximation; for it will appear in the next section that this abstract proposition is qualified by the inevitable imperfections of our statistical data.

($\gamma$) A third principle is that less weight should be attached to observations belonging to a class which are subject to a wider deviation from the mean. Such, in our case, would be the prices of articles which, exclusive of the common price-movement of all the selected articles, are liable to peculiarly large *proper* fluctuations. Cotton and Iron, for example, fluctuate in this sense much more than Pepper and Cloves.

The weighting of a geometric mean is a delicate matter, but not beyond the resources of science. A general rule is given by Dr. Macalister in the important paper already frequently referred to. Suppose we have a considerable series of observations belonging to a certain class, we can extract a constant which may be described as the measure of fluctuation for that series or class of observations. The constant thus given constitutes the *weight* with which we ought to affect the logarithm of an observation when we combine it, according to the arithmetic mean, with others (of a different degree of precision) in order to obtain the best possible measure. The data for determining this constant are afforded by series of prices for successive years, such as those in Mr. Giffen's *Report to the Board of Trade on Prices of Exports and Imports*, 1881–85.

If in the present state of statistics and public opinion it appears too difficult and delicate a matter to weight the data on the principle of fluctuation, the practical result of this section may be thus summed up. After the manner of Dr. Kraemer, select a number of (independently fluctuating) articles which are found to be particularly sensitive to changes in the value of money. After the manner of Jevons, find the percentage indicating the price-variation in each article, and put the geometric mean of those percentages as the required unit, or standard, or measure of depreciation. Or rather, if we must treat as equal weights certain to be unequal, it is better (for reasons which will be more fully stated in the next section) to employ a formula which is specially adapted to such jumbling of different weights: to wit, *the Median*. Examples of this species of Mean have been given above.

So far on the hypothesis that the widening circle of price-disturbance has not yet spread beyond a limited area; a case which is almost too restricted and particular to be the subject of our consideration.[1] If we suppose that the circle has completely

[1] Compare the last paragraph of the *Introductory Synopsis*.

spread, that all the compartments of the economic fabric are equally penetrated by the influence of some change in the supply of money, we have then a limiting case of the problem just discussed.

The objection to this supposition is that, for an all-pervading percolation, considerable time must, in general, be required. And then it happens—what is not necessarily true of more transient oscillations, such as those of an inconvertible currency—that the changes in prices are apt to be referable to one or two leading categories: *e. g.* of articles which follow the law of decreasing or increasing returns, after the manner exhibited by Laspeyres in his classical paper [1] on the prices of Hamburg wares.

If we examine some of the statistics adduced by Laspeyres, according to the appropriate mathematical methods, we shall not discover a very serious hiatus between the different categories of wares. The *modulus* for the fluctuation of the price-variations about their average may be (roughly) estimated to be about 40 for any of the eleven categories discussed by Laspeyres in the masterly paper entitled "Welche Waaren." . . . Hence we can calculate the probability that the differences between the various categories are really significant, and not merely accidental. It will be found, if, with Laspeyres, we dispose the data in three main divisions—*Urproductionen*, *Colonialwaaren*, *Manufacte*, etc.—that the cleavages *within* those divisions are not important. The separation between the divisions is marked, yet not very serious, not more serious than is found to exist within the most perfect groups which are known to exist; for instance, the proportion of male to female births. The mean (percentage) for the first division (*Urproductionen*), containing 129 wares, is 128; for the second division, containing 85 wares, 118; for the third, containing 98 wares, 108. The modulus of comparison between the first and second mean is (see the writer's "Methods of Statistics") about $40\sqrt{\frac{1}{129}+\frac{1}{85}}$ = about 5·5; while the observed difference is 10, nearly twice the corresponding modulus. Which constitutes a real, yet not enormous, difference; not greater than the differences in stature which exist between the sub-classes of a nation constituting a perfect type. Similar statements are true of the comparison between the second and third means.

If in the light of these conceptions we actually plot the 312

[1] *Jahrb. f. Nat. Oekon.* vol. iii. See also *Zeitschrift f. Staatswissenschaft*, 1872.

price-variations, it will be difficult to resist the impression that we have here a *typical mean* as perfect as any presented in concrete statistics, with the exception of the circumstance not relevant to the point now examined, that the curve representing the 312 wares, however continuous, and far from being saddle-backed, is not symmetrical about its greatest ordinate; the law of price statistics above announced making itself markedly felt.

The evidence that the general average rise for the whole group of 312 articles, namely, from 100 to 118, is no mere accidental appearance, but indicative of a real agency, is mathematically estimated by odds of trillions to one.

So nearly complete a fulfilment of our hypothesis is doubtless not presented by certain other statistics, *e. g.* some of those adduced by Dr. Forsell in his interesting brochure. But it may be safely said that no statistical argument would stand tests so severe as he applies. Consider the evidence in favour of the motion of the solar system, as marshalled in the masterly papers of Sir J. Airy and Messrs. Dunkin and Plummer in the *Memoirs of the Astronomical Society*. It will be found that, if you omit here, and stick in there, some star of peculiarly large apparent motion, the general conclusion as to the sun's movement will be most materially altered. *E pur si muove.*

• We see in the case of one example presented by one country that the hypothesis is fairly well realised by the price-variations of the majority of wholesale commodities. But it is a long step from one set of statistics to others, from wholesale commodities to the whole field of industry, and from a single country to the entire system of countries in monetary communication. Over a large area (as Leslie, Knies, and others have pointed out) there is apt to arise a marked diversity between the price-variations of different localities; a diversity which may well be inconsistent with the hypothesis of a unique and general mean type. There is no doubt that these considerations materially restrict the fulfilment of the conditions which are prefixed to this and the following section. It is possible, however, that an hypothesis, though known to be inexact, may correspond with the facts sufficiently well for the purpose in hand.

## Section IX.

*Determination of an index utilizing quantities of commodities: upon the hypothesis that a common cause has produced a general variation of prices.* (a f.) [1]

We have seen that, upon the supposition of a change in the supply of money, Jevons' method of combining the variations of prices without regard to the corresponding volumes of transactions is by no means so absurd as has been thought by some. The case is, as if we wanted to discover the change in the length of shadows, due to the advance of day. If the objects casting shadows were unsteady—waving trees, for instance—a single measurement might be insufficient. We might have to take the mean of several shadows. Now for our purpose the *breadth* of the upright object casting the shadow would be unimportant. The "wide-spreading beech" and the mast-like pine would serve equally well as a rude chronometer.

Suppose, however, that the top of the broader tree was not level but serrated, each apex oscillating more or less independently. If by the shadow of a tree was understood the mean length of the shadows cast by all its apices, in that case the broad tree should count for more than a bare pole. How much more would depend upon the connection between the projecting branches. The more independent the oscillations of each apex, the better the measure afforded by their mean shadow.

This image seems appropriate to our problem. Each price which enters into our formula is to be regarded as the mean of several prices, which vary with the differences of time, of place, and of quality; by the mere friction of the market, and, in the case of "declared values," through errors of estimation, it is reasonable to suppose that this heterogeneity is greater the larger the volume of transactions. On this account, therefore, and irrespective of those considerations of utility which were proper to our earlier sections, greater weight should attach to the prices of those commodities whose quantities are larger. It does not follow that the weights should be proportionate to the masses. The proper coefficients could be ascertained by scientifically examining the detailed statistics of each market. But it is agreeable to the Theory of Errors [2] and to the successful practice of

[1] In the preparation of this section the writer has derived much assistance from repeated conversations with Professor Foxwell.

[2] An improvement in weighting can only diminish, very often only slightly diminish, the error inevitably incident to the result of any measurement.

physicists to employ a discretionary good sense in assigning "weights" when a precise determination is difficult or impossible. In our case a good system of weights appears to be afforded by the quantities of commodities sold (once, and exclusive of resales) per unit of time. The weight so assigned would doubtless often be too large. It might sometimes be too small in the case of commodities much resold. On the whole it would be a good and safe system. This principle of ponderation is to be combined with those which have been given in the last section.[1] If we suppose the variation of prices not confined to a particular zone, but propagated over the whole sphere of industry, then we shall obtain a set of weights almost coincident with those prescribed (upon a different ground) by the standard based on National Consumption (Section III.). For the condition that the observations should be independent[2] leads us to exclude, or at least take little account of, the same commodity at different stages of production.[3]

But though in the present operation the weights would be much the same as before, the balance, the method of combination, is different. In view of the evidence adduced in the last section that price-variations are apt to be grouped asymmetrically, the "arithmetic" species of mean becomes precarious when our *quæsitum* is a quasi-objective type. The additional complexities which have been introduced in this section make against the geometric mean which was above recommended a certain hypothesis. There exists another species of mean more adapted to the rough character of our calculation, the Median; that is, in the simpler cases, that quantity which has as many of the given observations above it as below it, but a certain analogue of this operation, when the observations have different weights. *The*

[1] See the headings, $\alpha$, $\beta$, $\gamma$, pp. 243–4.

[2] See $\beta$, *loc. cit.*

[3] It would be a question whether industrial wages and industrial rent should be included, in addition to, and otherwise than as representative of, the corresponding products. At any rate their weights ought not to be proportionate to their volumes; partly on account of their close connection with commodities, partly on account of the magnitude of these volumes. In the case of transactions so extensive, and perhaps we may add some other large interests such as cotton and iron, it would be best to determine the proper coefficients by specially examining the detailed statistics of each market in the light of the Theory of Errors. A summary method would be to assign to these enormous masses an averagely large weight about as large as any other weight employed in our operation. The ideally best weight is not likely to be very different from the arbitrarily assigned one, and slight differences of weight do not appreciably affect the result; as may be seen by comparing the results corresponding to two different systems of weights.

*required formula is the Weighted Median*, the operation designated by Laplace [1] as the "Method of Situation."

The reasons in favour of the Median may thus be summed up. If, in spite of the evidence above adduced, the normal probability-curve should after all turn out to be the most appropriate representative of the group under treatment, the Median is a reduction well adapted to this case, affected as it is with a probable error only slightly larger than the arithmetic mean (Laplace, *loc. cit.* See "Problems in Probabilities," *Phil. Mag.*, Oct. 1886). But if the grouping is of the geometrical (Galton-Macalister) species, the Median is still a very good reduction, coinciding as it does with the greatest ordinate of the curve denoted. Moreover, it has been shown by the writer ("On the Choice of Means," *Phil. Mag.*, Sept. 1887) that there is a peculiar propriety in the use of the Median when the observations are "discordant," when their facility-curve may be regarded as a compound made up of different families, or different members of the same family, of symmetrical curves. It is now to be added that this prerogative of the Median is retained when some or all the discordant elements are of the geometrical species. Now the phenomenon of "discordance" is remarkably evidenced by the different degrees of dispersion which series of (*e.g.* yearly) price-returns present in the case of different commodities. Cotton, for instance, appears to have a much larger modulus of fluctuation than Pepper. Add that this method of reducing observations is the least laborious of all, and there will remain no doubt that in the present state of our knowledge, and for the purpose in hand, the Median is the proper formula.

The method of the Weighted or Corrected Median may best be described by an example. The first column of figures given below are price-variations, expressed as percentages, for nineteen commodities, obtained by comparison of the year 1870 with the period 1865–9. The figures are taken from table 26 of the Appendix to the Memorandum contributed by Mr. Palgrave to the Third Report on the *Depression of Trade*. The percentages given by him are here rearranged in the order of magnitude. Opposite each percentage in the third column is given the proportional quantity of commodity, or "relative importance," taken from Mr. Palgrave's table 27 (year 1870). The fourth column contains the (approximate) square roots of these quanti-

[1] *Théorie Analytique*, Supplement 2. See the present writer's paper on "Observations relating to Several Quantities," *Phil. Mag.*, 1887.

ties.[1] Now for the *simple* Median the rule is to find that one of the entries in column 2 which has as many observations above as below it: that is the *ninth* in the order of magnitude; which proves to be 94. For the *weighted* or corrected Median we still seek the entry in column 2, which has as many observations above it as below it; but we proceed as if the observation 71 had been made, not once, but 19·5 times; the observation 72 made 12·8 times, and so on. There being in all nearly 177 such constructive observations, the Median is the 89th, that is 94. Or in other

| Commodities. | Price-variations. | Quantities. | Square roots of quantities. |
|---|---|---|---|
| Cotton | 71 | 381 | 19·5 |
| Wool | 72 | 164 | 12·8 |
| Tobacco | 75 | 17 | 4·1 |
| Wheat | 80 | 418 | 20·5 |
| Copper | 82 | 30 | 5·5 |
| Coffee | 89 | 8 | 2·8 |
| Tea | 90 | 66 | 8·1 |
| Flax | 91 | 82 | 9 |
| Oils | 94 | 38 | 6·2 |
| Lead | 95 | 21 | 4·6 |
| Leather | 97 | 55 | 7·4 |
| Iron | 97 | 128 | 11·7 |
| Silk | 98 | 49 | 7 |
| Tallow | 101 | 44 | 6·7 |
| Meat | 102 | 382 | 19·5 |
| Timber | 104 | 150 | 12·2 |
| Indigo | 107 | 9 | 3 |
| Sugar | 120 | 143 | 12 |
| Tin | 120 | 15 | 3·9 |
| | | 2,200 | 176·5 |

words we have to find in the fourth column that figure which is such that the sum of all above [or below] it *with* the figure itself should be greater than half the sum of the entire column, but *without* that figure should be less than half the entire sum. The figure thus defined proves to be 6·2. For the sum of the entries above that figure is 82·3, and the half sum of the column is 88·25.

[1] The quantities of commodities taken as weights correspond to the *squares* of Laplace, $p_1$, $p_2$, $p_3$, etc. (*loc. cit.*). If we determine the Median by way of the third, instead of the fourth, column, we in effect assign for our system of weights the squares of the masses.* This operation, indicated by the bars in the third column, gives 91 as the Median. It is interesting to observe how small is the difference produced by the change of system—small in relation to the error incident to any Mean; which, as rudely estimated from the dispersion of the entries in the first column, is as likely as not to be as much as 2 or 3, and may not improbably be 4 or even 6. The difference between the systems is apt to be less, when the number of independent entries is greater. In the example cited from Mr. Giffen's statistics (where the number of entries is 58) the two systems of weights give *identical* results.

* In deference to physical analogies the term "mass" has sometimes been used in this connection where "volume of value" would have been more exact.

Now 82·3 is less than 88·25, while 82·3 + 6·2 is greater than 88·25. The entry in the second column which corresponds to the figure thus determined, viz. 94 (corresponding to 6·2), is the required *Weighted Median.*[1] The weighted Arithmetic Mean as calculated by Mr. Palgrave is 90.[1] By a similar operation performed on the export statistics for the year 1880, given by Mr. Giffen in his report of the year 1881, it is found that the Weighted Median (for the decline of price compared with 1861) is −7·8. Mr. Giffen's result, the corresponding Weighted Arithmetic Mean, is −5·83.[1]

The operation is much simplified by noticing that it is sufficient to arrange the percentages in the order of magnitude *in the neighbourhood of the Median.* For instance, if we are certain beforehand that the mean is below 100, we may dispose the entries above that figure in any order, just as they occur in the table from which they are taken.

We have shown how to construct a type of price-variations analogous to the *typical mean* of statures or other attributes defined as that height, or it may be weight, which appertains to a greater number of a certain population than any other height or weight does.[2] But here it may be asked, Why rest satisfied with a type if there exists a more substantial *quæsitum?* Why seek the mean variation of shadows instead of the objective movement of the bodies, that declination of the sun or revolution of the earth of which the varying shadows are the expression?* Why not penetrate beneath the superficies of shifting prices to the real relations between the quantity of money and commodities?[3]

The matter is simple as long as we keep to the abstract theory of the text-books. Imagine a purely metallic currency, the amount of which is, say, $Q$, and let the rapidity of circulation or duty of money be called $C$; then we may simply express the quantity of metallic money in terms of prices and volumes of transaction in our notation

$$Q = \frac{1}{C}[\alpha p_\alpha + \beta p_\beta + \text{etc.}].^4$$

[1] As to the import of these discrepancies see the preceding note.

[2] The Mean as defined in Dr. Charles Roberts' writings, not quite identical with Quetelet's *homme moyen* in case of asymmetrical curves like that on p. 239.

* This topic comes under the heading of the section inasmuch as the proposed calculation involves the quantities of commodities.

[3] What we have so far found is a mere ratio, comparable in point of objectivity to the ratio between male and female births (about 1,040 : 1,000 in England). But might the analogue be the proportion of black and white balls in large groups of balls which have been drawn at random from a huge urn? Beneath the tyical mean presented by those groups there is a more objective fact; the relative numbers of black and white balls, the masses of ebon and ivory.

[4] By $\alpha$, $\alpha'$, etc., for the purpose in hand we should understand not so much the amount of things sold as the amount of sales (per unit of time).

Now let prices vary with the quantity of money, other things being constant, and we have for the variation in the quantity of money the simple expression

$$\frac{\alpha p_\alpha + \beta p_\beta + \text{etc.}}{\alpha' p'_\alpha + \beta' p'_\beta + \text{etc.}} = \frac{Q}{Q'},$$

where $\frac{\alpha}{\alpha'}, = \frac{\beta}{\beta'} = 1$, etc., nearly, or upon an average.

Let us now introduce the several concrete circumstances, *first* that a proportion, say the ratio $K$, of transactions is effected by credit; *secondly*, that the volume of transactions varies between the epochs under comparison, say is multiplied upon an average by the factor $P$; *thirdly*, that the proportion of credit transactions, and *fourthly*, the duty of money, the coefficients $C$ and $K$, do not remain constant.

When we introduce the first attribute alone, no difficulty is felt. The factor $K$ disappears and leaves our formula in its initial simplicity. Again, when we introduce by itself the attribute of increased volumes, no great complication arises. We have only to multiply the simple formula by $P$ in order to obtain the diminution of metallic money relative to the volume of transactions, *per unit volume*, as one may say.

This proposition may appear at first sight still to hold good when we combine the two attributes hitherto considered separately. But this presumption is negatived by the fact that legal-tender money is largely used in modern industry, by way of *reserve*, to meet the residues of claims not mutually compensated. It is shown by the present writer in his paper on *The Mathematical Theory of Banking* [1] that, theoretically and abstractedly, reserves tend to vary as the *square root* of the volume of transactions which they support. The reserve of material money and the mass of credit transactions are to each other, as Mr. Giffen says, as the little weight and the big weight at the ends of the unequal arms of a lever. But it is a lever of a very peculiar mechanism, such that, when you increase the big weight, you lengthen the long arm. It will be understood, of course, that this doctrine is quite abstract and ideal; related to banking business very much as the "quantity theory" to hard-cash transactions—"the most elementary proposition," as Mill says of the latter theory, and without which "we should have no key to any of the others."

The proper factor, therefore, is no longer $P$. The mildest expression for the correction now required is of the form $(1 - K) P + KJ \sqrt{P}$, where $J$ is a new and probably unascertain-

[1] *Report of the British Association*, 1886.

able constant. That is theoretically [1] the sort of ratio by which, when the volume of trade increases, the mass of metallic money should be increased, in order to drive the trade at an unaltered level of price.

Now introduce the attribute that the ratio of credit to hard cash varies with time, and the varying ratio of the mass of metal to the volume of transactions, as we have good reason to believe.[2] Superadd the circumstance, which we have no reason to deny, that the rapidity of circulation also varies, and it is evident that the investigation which we have attempted is blocked by insurmountable statistical difficulties.

We might get a little further no doubt if we assume an additional datum, $R$, the ratio of gold in reserve to gold in actual circulation; then, with the help of $P$ and $K$ and $R$, as it were rail off from the industrial world a zone of hard-cash transactions to which the abstract formula of the text-books is applicable. This method has been pursued by Professor Neumann Spallart and Dr. F. Kral in the elaborate monograph *Geldwert und Preisbewegung*.[3] It certainly seems possible by this method [4] to explain the fact, if not to measure the magnitude, of a rise or fall of general prices; to predict the direction of the change, whether positive or negative, required in the amount of currency, in order that the level of prices may be restored.

It would be foreign to the spirit of this Memorandum to dwell upon ordinary statistical difficulties. But there is one scruple inherent in the nature of the metretic art which even with the progress of statistical technique does not seem likely to be removed.

[1] $K$ and $C$ being still supposed constant.

[2] *Cf.* Giffen, *Stock Exchange Securities*, "To give it [the abstract theory] validity, it must be assumed that a scarcity of money produces no expedients for economising money, and that an abundance of money does not lead to want of economy, which can hardly ever be the actual condtion of life."

[3] *Staatswissenschaftl. Studien*, n. v. Dr. Ludwig, Elster, Jena, 1887.

[4] The modest hope of explaining accomplished facts is not encouraged by Dr. Kral's success. For *a priori* he finds that the store of gold in Germany during the last few years has been fully adequate to the work which it has had to do—account being taken of rapidity of circulation and the amount of credit transactions. There have been no symptoms of a "Geldmangel," that is to say, no reason to expect a rise of the purchasing power of money, a general fall in prices. Yet *a posteriori* it seems to be admitted that there has been such a fall of prices. That this fall has originated "auf Seiten der Waren," that it is due to the development of industry rather than the introduction of the "Goldwährung" into Germany, may be a fact. But that fact does not seem to annul the right we have to expect a correspondence between the two lines of investigation; namely (1) the comparison between the supply of money and the amount required in order that the level of prices may be steady; and (2) the observed level of prices.

The method under consideration requires the determination of a certain residue, viz. total volume of transactions *minus* the portion effected by credit, $V - C$ in Dr. Kral's notation. Now each of these quantities, and *a fortiori* their difference, is subject to an error of measurement. And statistics must be much more perfect than there is any prospect of their being in the immediate future in order that the error incident to each of these measurables should not exceed a hundredth part of the same. But the hundredth part of the total transactions is a quantity of about the same order as that which it is sought to determine, namely, the amount of transactions in hard cash. The latter quantity, therefore, will be apt to be lost in a fringe of error. And, though the methods of determining $V$ and $C$ are likely to improve, yet the ratio of $V - C$ to $V$ or $C$ is certain to diminish, so that the precariousness of the calculation may well remain constant.

Upon the whole it seems that in the present state of science we must abandon the sort of realism which seeks an additional entity behind the phenomena of varying prices.* We must resign the fond idea of finding in the mean variation of price any quantity more objective than itself, any measure of its cause verifiable by an independent statistical investigation. We must be content with measuring the shadows; the objects behind them are beyond our reach. The cause of the observed phenomenon may be vaguely indicated as the changed relation between shining orb and opaque bodies; but there is wanting the mathematical science which should express the varying length of shadow as a definite function of the position of the sun.

The only question is whether we should not adopt a less, not a more, objective *quæsitum* than the type above described; whether, even where we can use the semi-objective type peculiar to this and the preceding section, it would not be better to use the more subjective formulæ investigated in the earlier sections. The present writer, following Laplace, has mainatined [1] that, even in the case of physical observations relating to a real thing, the proper method of combination is not so much that which is "most probably" correct, most frequently in the long run the true measure, but that which may "most advantageously" be employed.** *A fortiori*, when our *quæsitum* is at best a type, the

* The stone thus rejected has been made the corner-stone of a splendid edifice to Irving Fisher (see his *Purchasing Power of Money*, chaps. xi., xii.).

[1] *Metretike*, part ii.

** Cp. *Journal of the Royal Statistical Society*, 1908, Section I. Laplace applied the conception of "advantage" only to the determination of the best weights and species of average. It is here proposed to employ the principle of utility in

proper mean may well be not the ratio which is presented by the greatest number of (independently oscillating) prices,[1] but that ratio which in reference to human uses it is best to adopt in any general regulation.[2] However a peculiar importance may be attached to the character of objectivity, when the result of the investigation is to form the basis of action for Governments or International Conventions. It is fortunate that the difference between the two species of Means is likely to be inconsiderable numerically.

## Section X.

### *Mixed Modes; compounding the ends or means of several distinct methods.**

We have now examined all the branches represented on our tree. But we have by no means exhausted all the possible ramifications; for, according to the logic of compartments or combinations, six bifurcations—the number of our principles of division—lead to sixty-four distinct branches. It is further to be observed that two or more branches may unite to form a compound arm. Two or more separate objects may be simultaneously pursued. For instance, a Unit might be required which could combine the attributes $C$ and $c$, which should be adapted as far as possible to the convenience of the economic individual, both in his capacity of spender and earner. There might be sought the best possible compromise between the conditions that the creditor should receive a constant quantity of

order to determine that point in the—supposed stable but not in general symmetrical—frequency curve pertaining to the observations which should constitute the *quæsitum*, that point to which the process of averaging indefinitely prolonged would converge (*loc. cit.*, Section V).

[1] In the case of our metaphorical shadows suppose that the scope and end of the measurement was to ascertain whether and by how much shade for the use of man and his cattle was increasing or decreasing with the change of hour. The determination of a mean variation in the length of shadows would be useful only as a step towards that end. It would be better to aim directly at the end, and combine arithmetically the length of the shadows multiplied by the corresponding breadth; this system of weights being now determined, not on the principle proper to this section, but on the ground that the broader trees are the more umbrageous.

[2] Read Professor Foxwell's very able lecture on *Irregularity of Employment and Fluctuations of Prices*, and consider *what* it is, what sort of mean or function of prices, which he requires to be kept constant: whether it is what we have called the *Producers' Unit* (A B c), or some more objective mean of all price-variations weighted by the corresponding volumes of transactions.

* The logical symbols prefixed to this section in the original have been omitted as suggesting a composition of formulæ where there is only intended a composition of purposes.

value-in-use and that the debtor should pay an amount of money varying with his resources. This middle course might be designated by the symbol $A\ B\ (C + c)$. Or, if we start with the conception of a sliding scale, and base it partly on finished products, partly on other items (as materials or wages), we have the Mixed Mode $A\ B\ c\ (D + d)$.

Again, there seem to be combined in popular thought two elements which we have sought to distinguish in analysis, namely, the conception of an objective mean variation of general prices, and the change in the power of money to purchase advantages. It is as if having to measure the intensity of a drought we were to observe the decline of rainfall in every district over the whole country, and to take the mean of those observations; while at the same time keeping an eye to the fact that peculiar interest and importance attach to the decline of rainfall in certain regions, namely, those which constitute the catchment basins of the rivers which supply the population with water. The most comprehensive combination is that represented by our last symbol, purporting to be a compromise between all the modes and purposes [1] *—*the* method, if practical exigencies impose the condition that we must employ one method, not many methods.

Doubtless, practical wisdom lies in a mean, and compromise is of the essence of common sense. Some of the most useful plans and institutions are those recommended by a jumble of heterogeneous and incommensurable considerations, like the celebrated resolution [2] declaring the throne vacant after the flight of James II., of which Macaulay says that "its object was attained by the use of language which in a philosophical treatise would justly be regarded as inexact and confused. . . . The one beauty of the resolution is its inconsistency. There was a phrase for every subdivision of the majority."

There seems no more to be said, if what is required of us is a political measure rather than a scientific measurement. But, if otherwise, there is desiderated a *principle* by which to effect a synthesis between the purposes separated by our analysis. Perhaps

[1] Including many purposes which have not been thought worthy of a separate place here, for instance, to find the increase of National Wealth, given the total value at two epochs.

* *Cp.* Second Memorandum, Section III.

[2] "It was moved that King James the Second, having endeavoured to subvert the constitution of the kingdom by breaking the original contract between king and people, and, by the advice of Jesuits and other wicked persons, having violated the fundamental laws, and having withdrawn himself out of the kingdom, had abdicated the government, and that the throne had thereby become vacant." —*Macaulay*, chap. x.

it would be wisest frankly to acknowledge the arbitrary character of the proposed operation—

> "quæ res
> Nec modum habet neque consilium, ratione modoque
> Tractari non vult."

If a more definite answer is insisted upon, one might propose for imitation the Scotch practice of "striking the Fiars"[1] by means of a jury. A committee of experts agreed as to the general scope of the inquiry might be brought together, or put in communication.[2] Each member should independently form a numerical estimate based upon the data submitted to all. The *mean* of all these estimates constitutes the best possible value. It is thus that juries having to assess damages frequently proceed. The principle is illustrated by the following experiment. Ten gentlemen agreed each to guess the age of all the others and to state his own. The statistics so obtained evidence that a better estimate is afforded by the mean of several judgments than by the individual opinion. (For details see *Mind*, Jan. 1888).

No doubt it is a delicate problem in the higher *Metretics*, what degree of divergence in principle between authorities would be fatal to the collation of their judgments. Jurymen who differed materially as to the law or facts of a case could not with reason or advantage take a mean between their individual assessments. Similarly our monetary jury must be supposed to be agreed as to the general scope of the inquiry. Minor differences of opinion might be waived. The discrepancy between the various received formulæ for the Consumption Standard[3] would not be fatal, or rather would be favourable, to the combination of all the estimates into a mean result likely to be less fallible than any one of the measurements thus averaged. The methods of Messrs. Sauerbeck, Mulhall, Sidgwick, Marshall, Palgrave, Giffen, Lehr, and perhaps it may be added, Drobisch, and the one which is specially recommended in this Memorandum,[4] may be advantageously mixed. But, on the other hand, those who hold with the present writer that, in the construction of a standard for general purposes, a unique importance should attach to the items of National Expenditure—the average budget—the numerous adherents of this *Consumption-Standard*, might not consent to

[1] See W. K. Hunter's description of this practice.

[2] M. Dabos, in his *Etalon*, is perhaps the only writer who has frankly asserted that the value of gold is a metaphysical matter to be decided by cultivated intelligence.

[3] Above, p. 213.

[4] Above, p. 215.

merge an estimate so formed with the results of those who adopt a fundamentally different principle; for instance, Dr. Geyer's method, or another mentioned by him, which may thus be described. Take the price of each ware, just as it has been quoted. Add together these figures. The ratio between this aggregate at one epoch and the aggregate at another is put for the measure of the variation in the purchasing power of money.

The doctrine of the Mean, or principle of collated authority, admits of a certain analogical extension beyond mere arithmetical results to the determination of a function or form of combination. Accordingly that solution of our last problem, which is offered in the Report herewith printed, derives a certain confirmation, and the only sort of proof of which it is capable, from the general assent which it has received from the Committee of experts who have been appointed to consider this subject. A short analysis of that Report may fittingly conclude this Memorandum.*

The first part of the Report points out the necessity of distinguishing in theory several ends and methods [such as those which have been analysed in the preceding sections], the expediency of in practice giving precedence to some one mode [such as it is the main object of this section to discover.]

Part II., A, of the Report sets forth this mode, "the principal standard." It is a compromise between the principles of the Consumption-Standard, A B C D, and the more objective Mean, a f; an unequal compromise, inclined in favour of the first principle.[1] Agreeably to the first principle, yet without prejudice to the second,[2] the "weights" of the price-variations are the quantities of commodities. The form of combination, the "arithmetical" mean (or linear function), is prescribed by the first principle. In deference to the second principle, if not entirely on account of statistical exigencies, the prices used are wholesale prices, and the items of domestic service and residential rent have been excluded.

Part II., B, of the Report propounds six "subsidiary" index-numbers. Of these, three, *Wages*, *Workmen's Budgets*, and

* The Committee consisted of Mr. S. Bourne, Professor F. Y. Edgeworth (*Secretary*), Professor H. S. Foxwell, Mr. Robert Giffen, Professor Alfred Marshall, Mr. J. B. Martin, Professor J. S. Nicholson, Mr R. H. Inglis Palgrave, and Professor H. Sidgwick. The report, drawn up by the Secretary, and accepted with some amendments by the Committee, was published in the Report of the British Association for 1887.

[1] In giving these reasons the writer speaks only for himself.

[2] See Section IX. p. 247.

*Exports and Imports*, may be regarded as corresponding to those "partial interests," which were noticed at the end of the *Introductory Analysis* as of especial importance. Of the remaining three, the index-number based on *Wholesale Goods in General* may be perhaps put for the Producer's Standard, here designated A B c d E.[1] There remain the Consumption-Standard, A B C D,[1] and the Capital-Standard, A B c d e; the former pure and simple, the latter shorn of the item of labour, to which it may have some claim.[2] *

In concluding this paper, the writer desires to acknowledge gratefully that he is indebted for many important suggestions and corrections to his colleagues, the fellow-members of this Committee, especially Professor Foxwell.

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## THIRD MEMORANDUM.

ANALYSIS OF CONTENTS.



### SECTION I.

*Professor Newcomb's Method.*

One additional definition of the *quæsitum* which has come under the writer's notice since the completion of that Memorandum is that which has been propounded by the eminent mathematician Professor Simon Newcomb, of the Johns Hopkins University. He proposes to measure the variation in the value of the Monetary Standard by the change in the volume of value which is produced by the labour of an average individual in a unit of time.[3] He writes: "One possible hypothesis would be this. We might assume that the absolute value of everything produced by the population of the country remains unchanged, except that as a

[1] For convenience of reference the symbol B has been retained here; but the meaning would be more exactly expressed by omitting it, or substituting (B + b). We are not here concerned to distinguish whether the index-number is to be used as a *Standard for deferred payments*, or with some other view.

[2] See above, p. 230.

* There are omitted here, as rather hypercritical, some further remarks on the capital standard.

[3] *Principles of Political Economy*, Book III., ch. ii. § 10.

population increases the total value produced increases in the same ratio. In other words, we may suppose the average productiveness of each individual to remain the same from year to year."

Now this hypothesis may appear doubtful in the light of the statistics furnished by Mr. Edward Atkinson and others. There is reason to think that in an improving country the productivity of labour increases. But an intelligible rationale can still be assigned to Professor Newcomb's scheme considered as a standard for deferred payments. It may be regarded as just that the debtor should pay, the creditor receive, a constant proportion of the goods produced by an average man's labour. If the productivity of the average man increases, the creditor gains without the debtor losing. The principle may be illustrated by the present writer's proposal (in the former Memorandum) that the standard might be a constant proportion of the average income.[1] It is a principle which appears to be countenanced by some high authorities. Thus Sir Thomas Farrer, in his able Memorandum on *Gold and Credit* prepared for the Commission on the Precious Metals, asks: "If prices fall, not by reason of any change in the measure of value, but by increased abundance of the things sold, what considerations of justice or of convenience are there which call for an alteration in the measure of value?"

There is, however, a more important difficulty in the way of adopting Professor Newcomb's plan as a standard for deferred payments. Apparently there would be no distinction between articles of immediate consumption and those which are only agents of production; articles of each class would figure equally in the "value of everything produced" per year. Suppose that the national consumption might be divided into two classes of articles, one consumed nearly raw, the other elaborated through several stages of production, at each of which the transformed material changes hands by a mercantile transaction. Suppose the prices of the former category to rise on an average, while the prices of the latter category—both the long series of materials and with them the finished articles—fall on an average. It might

[1] Compare the definition of variation in the Monetary Standard which Mr. Giffen implies in the following passage of his important paper "On Recent Changes in Theories and Prices" (*Journal Royal Statistical Society*, Dec. 1888): "There may be a case of what may properly be described as depreciation of money where prices do not rise. . . . Measured by incomes, though not by the prices of commodities, there may unquestionably in such case be depreciation." *Cf.* Professor Walras's conception of a general diminution of the *rarity* (or final utility) of commodities. *Élements d'Économie politique pure*, Leçon xxxix. § 390.

happen that the value-in-use of the same monetary income,* say £100, would remain nearly constant for the average citizen. Yet, according to the new index-number, money might seem to be appreciated. Thus the annuitant or creditor might suffer, as he would receive, say, only £90 or £80 for every £100, if this scheme were adopted as a standard for deferred payments.

## Section II.

### *Professor Foxwell's Method.*

The conception of quantity produced, or rather sold, per unit of time has been embodied by Professor Foxwell in a distinct definition, which it was an omission on the part of the present writer not to have presented more clearly in the former Memorandum. Professor Foxwell is understood[1] to regard as the ideal measure of the variation in general prices an index-number which is based upon all vendible commodities whatever. He would make no distinction between articles of consumption and agents of production. In averaging the respective price-variations he would assign to each an importance proportioned to the corresponding value, or rather to that value multiplied by the number of times it changed hands (in a day, month, or year) by way of a monetary transaction. This plan is regarded as *par excellence* the measure of appreciation or depreciation.

If pressed with the objection which has just been addressed to Professor Newcomb, namely, that the index-number thus obtained is not the exactest possible measure of the change in the purchasing power of money experienced by the consumer, Professor Foxwell would reply that the consumer is not everyone. The interest of the producer, damnified by appreciation of money, is also to be regarded. The question set to us is a pure currency-question; and the answer to be sought primarily is, not by how much are debts to be scaled up or down, but by how much the metallic currency is to be multiplied in order that the monetary *status in quo* may be restored.

An extreme example may serve to bring out the character of the method. Suppose that the national consumption were divisible into two categories of commodities, the one involving

* This is hardly an objection to the method considered as a Production Standard.

[1] The present writer is responsible for the exposition and illustration of the views which he has obtained in the course of repeated conversations with Professor Foxwell.

only two mercantile transactions in their production, the other sold or re-sold some twenty times at different stages of its production. Suppose the prices of the former class drop on an average five per cent., while those of the latter drop as much as fifteen per cent., other things, and in particular the national taste, remaining constant. Then, according to the Consumption Standard, the index-number will be of the form $\frac{\frac{1}{2} \times 95 + \frac{1}{2} \times 85}{\frac{1}{2} + \frac{1}{2}}$; that is 90. But the new index-number may be written $\frac{\frac{1}{2} \times 2 \times 95 + \frac{1}{2} \times 20 \times 85}{\frac{1}{2} + \frac{1}{2} \times 20}$; that is approximately 86. This is not a Tabular Standard adapted to the interest of creditors and annuitants. It is the measure of the seriousness of appreciation for the community.

It will be observed that the example derives its force from the occurrence of a displacement in the rates of exchange between two classes of consumable articles; for without such displacement, if the drop of price in both categories were the same, there would be no difference between the results of the contrasted methods. Now (it may be said) such displacement is not one of the evils which "laws and kings can cause or cure." Let debtors and creditors regulate their private affairs by a special index-number if they like. That is not the affair of statesmen and financiers. But currency *is* within the province of government. It is competent to governments so to augment the currency, that the appreciation accused by the proper index-number may be reduced.

It should be explained that this scheme does not commit its propounder to any of the extreme views which in the former Memorandum[1] were connected with the conception of amount of sales and the work which gold has to do. He is not bound to refer to the quantity of gold actually existing in currency, or relative to an initial epoch. He need not pretend to calculate the amount of gold in use as money at present, or at the initial epoch. He need not pretend to calculate the ratio in which the quantity of gold at the initial period requires to be multiplied in order to equate the present with the original level of prices. He need not state the amount which for that purpose should be added to the present currency. What he professes to obtain is that ratio in which, if the quantity of currency were increased, other things remaining constant during the increase, the level of prices would be restored. But the amount of coin to be actually added is not

[1] Section IX.

necessarily deducible from the ratio thus conceived; because (1) the quantity of precious metal in use as money may not be ascertainable with any degree of precision, and (2) other things, in particular the condition of credit, may alter during the process of augmentation. In short our Professor is not to be confounded with the currency-quack who pretends to calculate the exact dose of currency which ought to be administered in order to keep the circulation in a healthy condition. Professor Foxwell's Index is rather of the nature of a diagnosis than a prescription; or at least it only enables him to prescribe the general character of the treatment—whether increased aliment or depletion—but not the exact quantity to be taken.

The *Currency Standard*, as Professor Foxwell's special *protégé* may be designated, is to be distinguished as follows from the *Consumption Standard*, which the Committee, in their collective capacity, have favoured. According to each method, the variation in the value of money is measured by a change in the monetary value of a certain quantity of commodity, supposed to be constant. But the standard quantity is not the same for the two methods. The choice is between the sum of valuables consisting of all the finished goods which pass into the hands of the consumer yearly, and that consisting of all goods whatever which change hands yearly. The basis of the one standard is, to use a bold phrase, the mass of final utility; the basis of the other standard is, to use a bolder phrase, the momentum of final utility.*

Upon the whole, it appears that the Currency Standard deserves more attention than it has received. The stone unaccountably set aside by former builders of index-numbers may become the corner-stone of future constructions.

It is not to be thought because the proposed method is likely not to be so revolutionary in practice as it is distinctive in speculation,[1] that therefore it is unbefitting a separate and high place here. For we are concerned here with distinctions of method rather than differences of result. There is attempted here—to

* There is here omitted a lengthy illustration which was introduced in the original with the design of making the conception of the Currency Standard clearer. But further explanation seems superfluous now that Professor Irving has made familiar the cognate conception which he designates MV. The reference may remind us that for the purpose of ascertaining the change in the value of money the Currency Standard as above formulated requires to be divided by a denominator corresponding to Professor Fisher's "T."

[1] Thus the example which we have imagined is probably an extreme one; yet it presents a difference between the compared index-numbers of only seven per cent.; which, in view of the "probable error," say two or three per cent., to which *any* index-number is liable, cannot be considered as colossal.

illustrate small things by great—for a particular province of industry, the sort of analysis which an eminent member of our Committee has performed upon the "Methods" of conduct in general. In the sphere of Finance, as well as Ethics, theoretical distinctions are important, although they may not correspond in practice to such marked discrepancies as might have been expected.

Nor is it a fatal objection to the scheme that it would be impossible to ascertain with precision the proportions in which each commodity absorbs, or exercises a pull upon, the currency; that here the number of resales, and there the exceptional use of credit, would defy calculation. For, regarding the proposed index-number as a Weighted Mean of numerous given variations of price, we see that the objection amounts to saying that the weights are liable to a considerable error. But, as shown in a former Memorandum and to be insisted on again in the present one, the erroneousness of the weights is likely to produce much less error in the computed mean than might have been expected.

Nor is it to be objected that, in the present state of statistics, it would be impossible to obtain returns under several of the headings, that many important articles would have to be omitted altogether. For the plan still may present an ideal in the direction of which it may be thought advisable to move as far as possible. It may supply a rationale to some practical method. Thus, any large aggregate of miscellaneous articles, finished and unfinished, may be regarded as a sample taken at random from the immense incalculable series which forms the data of the ideal index-number. For instance, such a sample may be afforded by the statistics of foreign trade, which we now proceed to consider.

## Section III.

### *Mr. Giffen's Methods.*

The next solution of our problem which calls for some additional remarks is that which is deduced from the Statistics of Foreign Trade. It is proposed first to examine the principles upon which Mr. Giffen's masterly calculations are based.[1]

The primary object of the whole investigation appears to have been to compare the volume of trade in different years.[2] The purpose is, in the language of this Committee's first Report, to

[1] *Parl. Papers*, 1878–9, C 2247; 1880, C 2484; 1881, C 3079; 1884–5, C 4456.

[2] Consider the title and introductory sentences of the Reports.

enable us, "given the increase of value [of exports or imports in one year as compared with another], to estimate the increase in quantity of the class of commodities under consideration." *

But there is room for casuistical discrimination when we inquire what is the meaning and measure of increase in the volume of trade or quantity of commodities.

At first sight the following method of comparing the volume at different epochs might seem plausible. Compare the (given) quantity of one article, say $a$, in one year, say year $x$, with the quantity of the same commodity in the compared year $y$. We thus obtain a ratio

$$\frac{\text{Quantity of commodity } a \text{ in year } y}{\text{Quantity of commodity } a \text{ in year } x}.$$

Form now a similar ratio for article $b$, and again for $c$, and so on. The circumstance that the unit of $a$ is avoirdupois, that of $b$, it may be, liquid measure, and so on, need not clog these calculations of ratio. We shall thus obtain as many ratios as there are articles, say fifty, as approximately in some of Mr. Giffen's computations. Now take the mean of these fifty ratios. That mean ratio represents the variation in the volume of trade between the years $x$ and $y$.

This solution of the problem is by no means to be despised as naïve. It presupposes no doubt a certain sympathy and conformity to a common type on the part of several augmentations of which an average is taken. But it will be shown that this hypothesis is adequately verified.

As to the first point, consider the figures on p. 266, which are obtained by dividing the quantity of every export in 1883 by the corresponding quantity in 1880. The quantities are taken from Mr. Giffen's Table V., Part I.;[1] and the quotients are given in the order in which those quantities occur. Thus for *Alkali* the quantity (of cwts.) is for 1880, 6,888, and for 1883, 6,947; the figures after the first four being neglected. Accordingly, the quotient is 1·01, or 1·0. The figures are true to the first place of decimals.

The grouping of these ratios is exhibited in the diagram on p. 267; where each upright line, surmounted by a figure

* The implication between change in general prices and change in total quantity which is involved in the construction of an index-number such as that considered in this section is well exhibited by Professor Irving Fisher in his *Making of Index-numbers*.

[1] "Reports on Recent Changes in the Value of Foreign Trade, *Parl. Papers*, 1885, C. 4456; and 1888, C. 5386, Part III. Table 2.

expressing its length, represents the number of times that a certain ratio occurs. Thus the ratio 1·1 is presented eleven times; the ratio 1·2 ten times. The median is 1·1; a result which agrees with that obtained by Mr. Giffen's more elaborate and accurate method. He, in effect, weighting each of these ratios with the values of the corresponding article for 1883, finds for the ratio of the volume of 1883 to the volume of 1880 the quotient 146,371,015 ÷ 138,032,674 = 1·06, or approximately 1·1.

| Ratios of Quantities in 1883 to Quantities in 1880. | Continued. | Continued. | Continued. |
|---|---|---|---|
| 1·0 | 0·6 | 1·1 | 1·2 |
| 1·4 | 0·8 | 1·0 | 0·9 |
| 1·0 | 1·2 | 1·1 | 1·4 |
| 1·1 | 1·0 | 0·4 | 1·0 |
| 0·9 | 1·0 | 1·0 | 1·0 |
| 2·2 | 1·1 | 0·7 | 1·3 |
| 1·3 | 1·4 | 1·3 | 1·2 |
| 1·0 | 1·1 | 1·1 | 1·1 |
| 1·1 | 0·9 | 0·9 | 1·3 |
| 1·4 | 1·3 | 1·1 | 0·9 |
| 1·2 | 1·2 | 1·1 | 1·0 |
| 1·2 | 1·2 | 1·3 | 1·0 |
| 1·4 | 1·1 | 1·2 | 1·2 |

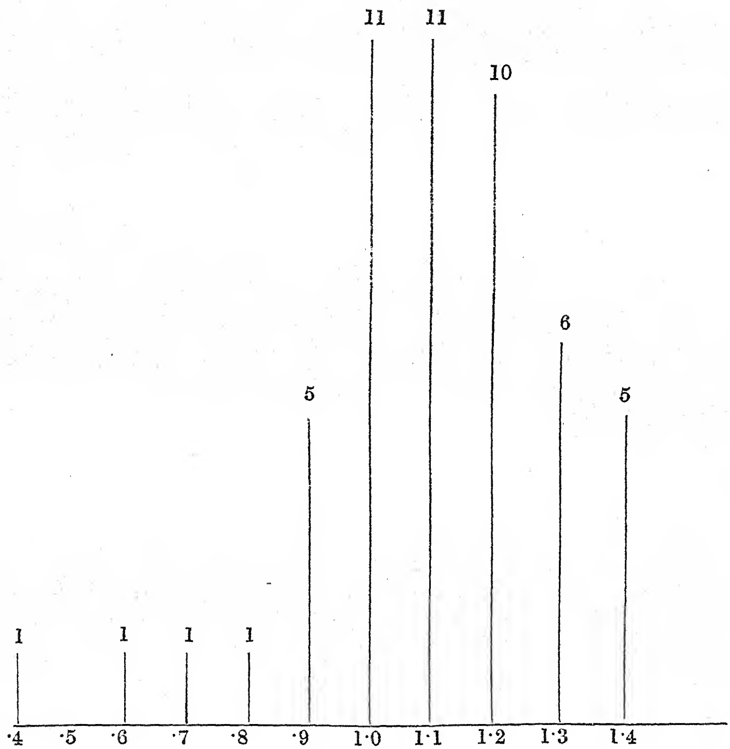
Again, comparing 1886 with 1883, and taking each of the fifty-two ratios to two decimal places, I find for the median 1·00; while Mr. Giffen's Weighted Mean is ·98.

This consilience might have been predicted by the Calculus of Probabilities, if cotton and perhaps one or two other articles whose values constitute abnormally large weights had been omitted.[1] The fact that even without that omission the results coincide shows an even greater symmetry in the movement of trade than might have been expected.

This problem, as we have seen, presents a variety of phases. But for the particular purpose in hand it will be sufficient to make two divisions. First, we may suppose the variation in the Monetary Standard ascertained by examining a wider sphere of industry than foreign trade, or we may confine ourselves to the statistics of exports and imports. The first alternative has not been entertained by Mr. Giffen in his Reports; and it will be dismissed here as leading back to varieties of our problem which have been already considered. Again, the following distinction may be taken. In combining the comparative prices or ratios between the prices of each article at two compared epochs we

[1] The verification holds good when one, or more than one, of the returns for *cotton* are omitted.

may assign a certain weight to each ratio proportioned to the importance of the corresponding commodity, or else we may suppose a change in the level of prices propagated over a whole zone of trade in such wise that we may take a simple average of the given ratios without attending to the corresponding masses of commodity. For a further enunciation of this hypothesis the reader is referred to the former Memorandum, and to the sixth section of the present one. Of these alternatives Mr. Giffen has adopted the former.




It is submitted that this course commits us to some such hypothesis as the following. If, in order to compare the volume of trade for a series of years, we assign a weight to the price of each article proportioned to the importance of that article, we must regard the relative importance of each article as constant for that series of years. If, then, the relative importance of each article is to be measured by the pecuniary value of the quantity bought or sold, the proportions which the value of each article

bears to the value of any other article or to the total value of all the articles, ought to be pretty constant during the whole series of years. This assumption is strikingly verified by Mr. Giffen's Table II. Again, if the proportionate amount expended on each article is pretty constant from year to year, we may conceive a purchasing public (whether the community in whose interest the computation is being made, or the foreigners with whom they deal) constant as to the nature of their wants [though it may be increasing in numbers in the course of years]. Accordingly the rates of exchange between the different articles ought to be constant. In other words, the ratio between the prices of the different articles ought to be constant during the series of years. This assumption is verified as well as could be expected by Mr. Giffen's Table I. and Table III., A.[1]

The values and prices being constant, it is implied that the proportionate quantities also, the number of tons, or it may be gallons, of each article exported or imported, have a degree of constancy. This proposition also may be verified by glancing at the quantity columns in Mr. Giffen's Table IV., or the same figures in the statistical abstract.

These assumptions as to the steadiness of the course of foreign trade being admitted, a definite interpretation may be assigned to the otherwise vague idea of increase in the volume of exports and imports. Or rather two or three definitions become possible. The primary significance of an increase in the volume of foreign trade is as a measure of the benefit which the community desires from foreign trade.[2] This conception is particularly germane to the case where the articles on which the computation is based are

[1] There are reasons why Mr. Giffen's table of price variations (Table III., A) should present the appearance of stability in a less degree than his table of proportionate values (Table II.). First, each entry in the former table is obtained by comparing one item with another item, viz. the price of an article in any year with the price of the same article in 1861; whereas each entry in the latter table is obtained by comparing an item (the value of an article) with an *aggregate* (the total value), which of course is apt to be more stable than an item. If the suggestion made below of referring each price to the mean price of the article for adjacent years were adopted this contrast would doubtless be diminished.

[2] The variation in the volume of trade as thus conceived is very similar to Cournot's definition of "real gain," or loss, of social revenue (*Recherches sur les Principes Mathématiques de la Théorie des Richesses*, ch. x.; and later redactions). But Cournot, who seems not to have seized the idea of "final utility," strains the monetary measuring-rod beyond its legitimate application when he propounds his paradox that freeing a commodity from a prohibition results in a loss of real gain to the country which becomes an importer thereof (*ibid.* Art. 89). For this case implies a change in the *quality* of trade, a diversion of the streams of commodity into new channels with which our methods are unable to deal, through failure of the hypothesis enunciated in the text.

commodities imported for the consumption of the community. "In some countries," writes Mr. Giffen, "the whole imports less the re-exports may be treated as imports for final consumption." If the imports are materials as distinguished from finished products, still the unfinished articles may be taken as more or less perfect representatives of consumable commodities.

The case of exports may be thus fitted to this interpretation. It is to be assumed that, given the steadiness in the course of trade which we have postulated, an increase in the volume of exports normally corresponds to an increase in that of imports. Thus exports afford a measure of the advantage derived from foreign trade of the same sort as that which imports afford.[1]

There is a special difficulty in the case of those articles which are imported like cotton in order to be re-exported at a subsequent stage of manufacture. Take the extreme case, mentioned by Mr. Giffen, of tea, which figures as part of the domestic produce exported from France. The French of course derive some advantage from the handling of this article. But the interest which they have in the tea thus transmitted is not proportioned to the value of the article in the same sense as the value of a genuinely native export measures its importance to the nation.

With regard to this special difficulty, and indeed the whole computation, it is to be remarked that we are concerned—not so much with the absolute volume of trade—as the relative volume in one year as compared with another. The relative volume as already stated may be regarded as a sort of mean of the ratios between the quantity (in tons, gallons, etc.) of each commodity in one year and the corresponding quantity. It is a weighted mean, the weights being the respective values of the commodities.[2]

[1] It may be objected that the volume of trade is *per se*, and apart from hypothesis, an interesting datum, or rather *quæsitum*, as affording the measure of profits accruing to the country, or for some such reason. This remark seems just if the corrections of the Monetary Standard which are made for the purpose of estimating the volume of trade are based upon some principle extraneous to the trade, or at least some other principle than that of assigning to each article an importance proportioned to the value exported or imported. All that is contended here is that the received method of measuring the trade by itself, so to speak, postulates a certain analogy between this species of index-number and the more general one which is based on national consumption. Indeed, it is partly on account of this analogy that the subject appears to deserve such full treatment here.

[2] In the symbols to be presently introduced the ratios of quantity are of the form

$$\frac{q_{ay}}{q_{ax}}, \frac{q_{by}}{q_{bx}}, \text{ etc.}$$

The corresponding weights are $q_{ax}\, p_{ax}$, $q_{bx}\, p_{bx}$, etc. Thus the weighted mean is

$$\frac{q_{ay}\, p_{ax} + q_{by}\, p_{bx} + \text{etc.}}{q_{ax}\, p_{ax} + q_{bx}\, p_{bx} + \text{etc.}}$$

Grant, now, that in the proposed case of tea transhipped from France the weight is exaggerated. Yet, as pointed out by the writer in a former Memorandum, some inaccuracy of the weights is not likely to affect the result much. It is only in the case of the larger values, notably cotton imported into the United Kingdom as the material for future manufacture, that the difficulty is serious. Such items ought no doubt to be placed in a separate category, and considered on their own merits; not merely on account of their possible inaccuracy, but also on account of their mere magnitude. The domineering pre-eminence of one or two items is fatal to the application of the Calculus of Probabilities which flourishes, so to speak, only in a republic of numerous independent not very unequal constituents.

Implicated with this definition of the volume of trade there is a definite method of measuring the variation in the value of money.* This method is of the same general character as that proposed by the Committee, but more partial and imperfect, as concerned only with a fraction of the national consumption, and that fraction often very indirectly represented.

A slightly different conception of the method may be distinguished by an exhaustive casuistry. The measure of the variation in the value of money, which is afforded by the statistics of foreign trade, may be of the species which was defined in the third section of the former Memorandum. This is a standard, adapted indeed to deferred payments, yet for which the items entering into the index-number " are not copied from the statistics of national expenditure, but are selected on some other principle." It is presumed in virtue of the general sympathetic movement of prices that the change in value of the articles of national consumption is adequately represented by the change of value in certain other articles selected on what may be called a random principle from the whole mass of trade. Whichever of these two slightly distinct views we take, we may perhaps describe the principle of measurement as a *quasi*-Consumption Standard.

This reference to first principles is by no means otiose. It assists in deciding what differences of method are fundamental, how far our choice may be governed by regard for mere elegance and ease, and so we may say of rival methods—

" Whate'er is best administered is right."

Thus it will be maintained that Mr. Bourne's dissent from

* See the new note at the beginning of this section.

Mr. Giffen's practice is not justified by first principles. On the other hand, reasons will be given for differing from the opinion which Mr. Giffen seems to entertain, that his second method, set forth in the fourth table of his earlier Reports, is less serviceable than the method to which his first three tables refer.[1]

The discussion of the questions raised may be facilitated by the use of symbols. Let $a$, $b$, $c$, etc., denote the commodities of which we are given the quantities and prices for a series of $n$ years. Let the successive years be designated by the numerals 1, 2, 3, etc. Let $q_{a1}$ be the quantity (imported or exported) of the commodity $a$ in the year 1; $q_{b1}$ the quantity of commodity $b$ in the same year, and so on. And let $q_{a2}$, $q_{b2}$, etc., represent the quantities in the year 2, and so on. The absolute magnitude of the quantities of commodity increases in general from year to year; but the *proportions* between the respective masses of commodity are, by hypothesis stated at page 267, constant. Thus we are to imagine each set of ratios $q_{a1} : q_{b1}$, $q_{a2} : q_{b2}$, etc., $q_{an} : q_{bn}$, as quantities of the same order hovering about a mean or diverging from a type which we may denote by $\gamma_a : \gamma_b$. Similar suppositions are made with respect to the other articles.* The annexed arrangement of the symbols brings these relations clearly under view :—

| | Article $a$ | Article $b$ | | Article $r$ |
|---|---|---|---|---|
| Year 1 | $q_{a1}$ | $q_{b1}$ | . . . . . | $q_{r1}$ |
| Year 2 | $q_{a2}$ | $q_{b2}$ | . . . . . | $q_{r2}$ |
| . | . | | | |
| . | . | | | |
| . | . | | | |
| Year $n$ | $q_{an}$ | $q_{bn}$ | . . . . . | $q_{rn}$ |
| | $\gamma_a$ | $\gamma_b$ | | $\gamma_r$ |

Here each $\gamma$ stands for any of the $q$'s in the column above it; or, rather, the ratio of any one $\gamma$ to another; *e.g.*, $\gamma_a : \gamma_b$ stands for the ratio of the corresponding $q$'s for any year.

Similarly let $p_{a1}$ denote the price of the article $a$ in the year 1, $p_{a2}$ the price of the same article in the year 2, and so on. Here the absolute magnitude of the prices varies from year to year with the appreciation or depreciation of money; but the proportions between the prices of the respective commodities are regarded as fairly constant. Then we have a scheme for the $p$'s like that of the $q$'s :—

[1] *Parl. Papers*, 1878–9, C. 2247, p. 4, par. 4.

* The intrusion of assumptions proper to the theory of *Sampling* should be noted.

| | Article $a$ | Article $b$ | | Article $r$ |
|---|---|---|---|---|
| Year 1 | $p_{a1}$ | $p_{b1}$ | . . . . | $p_{r1}$ |
| Year 2 | $p_{a2}$ | $p_{b2}$ | . . . . | $p_{r2}$ |
| . | . | | | |
| . | . | | | |
| . | . | | | |
| Year $n$ | $p_{an}$ | $p_{bn}$ | . . . . | $p_{rn}$ |
| | $\pi_a$ | $\pi_b$ | | $\pi_r$ |

Here each of the $\pi$'s is typical of the column above it; or, rather, the ratio of any one $\pi$ to another is typical of the ratio between the corresponding $p$'s for any year.

Upon these hypotheses the following appears to be the most general, or at least a sufficiently general, representation of the proportionate volumes of trade for the series of years.

Volume of imports or exports in year $x$ is proportional to value of imports or exports in year $x$ ÷ index-number indicating the ratio of the price-level in the year $x$ to the level of prices which is taken as standard.

∴ Volume of imports [or exports] in year $x$ is proportional to value of imports [or exports] in year $x$

$$\div \frac{\gamma_a p_{ax} + \gamma_b p_{bx} + \text{etc.}}{\gamma_a \pi_a + \gamma_b \pi_b + \text{etc.}},$$

where, as already explained, $\gamma_a$, $\gamma_b$, etc., $\pi_a$, $\pi_b$, etc., are the typical quantities and prices.

So far we have supposed both the quantities and prices of the articles imported or exported to be given. In the concrete case where these data are wanting for a considerable set of articles of which the value only is given, the formula is still the same, viz., Volume in year $x \propto$ Value in year $x$ ÷ index-number indicating ratio of the level of prices in the year $x$ to the standard level. The only difference is that we must now base our index-number upon a part only of the total trade whose volume is required, assuming that what is true of a part is true of the whole.

How now are we to determine the *types* which enter into our formula? First as to the quantities. The most obvious course is to take the $q$'s of a particular year for the typical $\gamma$'s, *e.g.*, $q_{ax}$ for $\gamma_a$, and so on. In the absence of special reasons in favour of or against certain years, we may select any one of the $n$ years to furnish the typical quantities. We have thus at once $n$ different schemes. But it need not be postulated that the same system of quantities should be adopted for each of the series of years. In fact, in the scheme of Mr. Giffen's Table IV. different factors are employed for each comparison, namely, the factors furnished by each year which is being compared in respect of its level of

prices with the standard year (1861). If this additional liberty is used to its full extent for every one of the $n$ schemes already enumerated, we have now $n$ variants; that is, in all we have $n^2$ different formulæ. However, it may be admitted that these additional schemes, with the important exception of the particular one used by Mr. Giffen in his Table IV., are, if not less accurate, at least less elegant than those which were mentioned first. We shall therefore dismiss these variants with the exception of that one which seems peculiarly appropriate. So far, then, we have $(n + 1)$ schemes presented by the varieties of the quantity-types, the price-type being supposed fixed.

But the price-types also are manifold. A system of such types is furnished by the actual prices of every year—in the absence of special reasons against some particular year. Thus Mr. Giffen has chosen 1861 as the year of standard prices, Mr. Bourne 1883. We have thus $n$ additional cases, which, compounded with the $(n + 1)$ above ground give $n\ (n + 1)$ distinct schemes or formulæ for comparing the series of volumes.

Out of this whole number there are $2n$ which deserve particular attention, namely, those in which the quantities or factors employed in each comparison are supplied by one of the compared years. One system of such schemes in number $n$ is obtained by using in every comparison the factors supplied by the year of standard price; in other words, by taking the types of price and quantity from the same years. The other system, also numbering $n$, is that which was noticed in the last paragraph but one as having been used by Mr. Giffen in his Table IV. The quantities in this system are supplied by the year which is being compared in respect of its level of prices with the year of standard price. Of course, if we were concerned with only one comparison at a time, if each comparison were an independent operation, these selected schemes would be entitled to a decided preference. But where the object is to find a series of numbers, representing by the ratio of any one to any other the proportion between the volumes of trade for the corresponding years, there seems to be no advantage in constructing our measuring-rod with the factors of one year rather than another. The whole computation presupposes some such hypothesis as that which has been enunciated above; and on that hypothesis one year has no claim to be preferred before another.

What may be said in favour of the selected schemes is that they are very slightly more convenient than the other ones. In general, it may be observed that we have $n$ operations, each of a

kind illustrated by the formation and addition of the columns in Mr. Giffen's Table III., B. One such operation is required to construct the denominator of the index-numbers which express the ratio between the level of prices in the standard year and each other year. The denominator is in general terms, as we have seen, $\gamma_a \pi_a + \gamma_b \pi_b +$ etc; or, if we take the $\gamma$'s from one year, say $x$, and the $\pi$'s from another year, say $y$, the denominator becomes

$$q_{ax} p_{ay} + q_{bx} p_{by} + \text{etc.}$$

To be compared with this denominator there are $(n - 1)$ numerators, one for each of the years except the standard one, each numerator of the form

$$q_{ax} p_{az} + q_{bx} p_{bz} + \text{etc.}$$

where $z$ is a year compared with $y$ in respect of the level of price.

There are, in general, then, $n$ such operations: $(n - 1)$ for the numerators, and one for the denominator. But in the particular case where the types of price and quantity are taken from the same year, where $x = y$, the denominator reduces to

$$q_{ax} p_{ax} + q_{bx} p_{bx} + \text{etc.} = \text{Value for year } x,$$

a given figure which requires no computation. Accordingly one operation—that of calculating the denominator—is spared. Again, if we take the factors from the particular year, say $z$, which is being compared in respect of the level of prices with the standard year, that is, if in the last paragraph we put $x = z$, the numerator reduces to

$$q_{az} p_{az} + q_{bz} p_{bz} + \text{etc.}$$

forming the total value for the year $z$, which is a given figure. But meanwhile, in employing a different scheme of factors for each numerator, we have necessitated the use of $(n - 1)$ different denominators, each of the form

$$q_{az} p_{az} + q_{bz} p_{bz} + \text{etc.}$$

The valuation of each of these forms will require $(n - 1)$ operations of the kind described.

In addition to this slight advantage in respect of ease there may also be ascribed a peculiar elegance to the selected formulæ. But they have no claim to the highest degree of accuracy. That distinction belongs to a more complicated system, which is now to be described. We have so far taken for granted that each typical quantity $\gamma$ is furnished by some $q$ which is the actual quantity for a particular year. But it is more agreeable to the

Calculus of Probabilities to take some *Mean* of the given $q$'s for the type $\gamma$. This principle has been recognised in the First Report of the Committee, where it is proposed that for the purpose of comparing the level of prices at two different epochs the factors employed should be the mean of the respective quantities. In the variety of the problem with which we are at present concerned we may suppose a whole series of corresponding quantities, *e.g.*, for the article $a$ $q_{a1}$, $q_{a2}$, etc., $q_{an}$. The mean of all or any number of these quantities may be taken for our type. Now, out of $n$ quantities $(2^n - 1)$ distinct combinations may be formed. Instead, therefore, of the $n$ different arrangements of factors which we at first found we have now $2^n - 1$; which, being combined with the one peculiar scheme employed in Mr. Giffen's Table IV., makes $2^n$.

Similar remarks apply to the types of price. We have so far taken actual prices for our types. But it may be better to imagine a sort of mean year with normal or typical prices formed by arranging the actual prices of several years. This principle has been employed to some extent by Jevons, Dr. Soetbeer, Mr. Palgrave, and Mr. Sauerbeck. In virtue of this principle the $n$ different bases of price which we found before are swelled to $2^n - 1$. Altogether, therefore, we have $2^n \times (2^n - 1)$ distinct schemes of index-number.

This account may be further multiplied if we have a choice, as would often be proper, between the Arithmetic Mean and a certain other species of average which is noticed below. However, it may be well to leave some margin for the occurrence of abnormal years (like 1873) whose data cannot be used freely. So let us be content with the modest estimate just furnished, as resulting from that degree of liberty of choice which we have so far contemplated.

That is, however, a very narrow view. For each $q$ which we have employed may be replaced by an expression which is by hypothesis *of the same order*. For instance, we are entitled to put for $q_{ay}$, $q_{by}$, etc., the expressions $q_{ay}\frac{p_{ay}}{p_{ax}}$, $q_{by}\frac{p_{by}}{p_{bx}}$, etc.; say $q'_{ay}$, $q'_{by}$, etc. This is, in effect, what Mr. Giffen has done in the classical computations comprised in the first three tables of his reports. His formula for the volume of any year, $z$, may be written in our notation :—

$$\text{Volume of year } z \propto \text{Value of year } z \div \frac{\left(q_{a75}\frac{p_{a75}}{p_{a61}}\right)p_{az} + \left(q_{b75}\frac{p_{b75}}{p_{b61}}\right)p_{bz} + \text{etc.}}{\left(q_{a75}\frac{p_{a75}}{p_{a61}}\right)p_{a61} + \left(q_{b75}\frac{p_{b75}}{p_{b61}}\right)p_{b61} + \text{etc.}}$$

The reader will easily see the equivalence of this formula to that which Mr. Giffen has made familiar, if the symbols $p_{a_{61}}$, $p_{b_{61}}$, etc., are brought outside the brackets both in the numerator and denominator. The denominator, for instance, will become $(q_{a_{75}}\,p_{a_{75}}) + (q_{b_{75}}\,p_{b_{75}}) +$ etc.; corresponding to the column headed 1875 in Mr. Giffen's Table II.

By parity it may be shown that the index-number constructed by Mr. Palgrave implies the following formula for volume :—

$$\text{Volume of year } z \propto \text{Value of year } z \div \frac{\left(q_{az}\dfrac{p_{az}}{\pi_a}\right)\times p_{az}+\left(q_{bz}\dfrac{p_{bz}}{\pi_b}\right)p_{bz}+\text{etc.}}{\left(q_{az}\dfrac{p_{az}}{\pi_a}\right)\times \pi_a+\left(q_{bz}\dfrac{p_{bz}}{\pi_b}\right)\pi_b+\text{etc.}}$$

where $\pi_a$, $\pi_b$, etc., are types of price obtained by taking an average over certain years.[1] In fact, Mr. Palgrave's scheme may be regarded as a variant of the plan employed in Mr. Giffen's Table IV., which was above commended to particular attention.

These $q''$s may be combined with each other in the same way as the $q$'s; and, indeed, the $q''$s and the $q$'s may be mixed. However, these operations would be laborious and inelegant. We shall, therefore, cull from the infinite field which has just been opened up only just such a number as to double the estimate already reached. It may be useful to show how this additional contingent is reached, taking as a conspicuous instance the materials of Mr. Giffen's work. It was open to him to have taken for the basis of prices some year other than 1861. In fact, in his fourth table he has so used both 1873 and 1883. Or the base line might have been composed by taking the average prices of each article for several years, after the manner of Mr. Palgrave or Mr. Sauerbeck, except that the years entering into the average need not be consecutive. It may be asked, What reason could there be for taking half-a-dozen years—some at the beginning, it might be, and some in the middle, or at the end, of the period under review? The reason might be the very absence of a reason. Suppose it were thought desirable, in order to avoid accidents, to take a mean of half-a-dozen years, and not worth the trouble of including more than half-a-dozen. In the absence of special objections to certain years any one half-dozen is as good as any other. There are, therefore, as many half-dozens as there are combinations of six to be formed out of the $n$ years. To avoid the suspicion of cookery it might be best to make a selection at

[1] See, on Mr. Giffen's and Mr. Palgrave's index-numbers, Section II. of the present writer's first Memorandum, *Brit. Assoc. Report,* 1887, p. 265.

random—by spinning a teetotum, or by some equally arbitrary process. It should be observed that the labour of taking averages over several years need not be so formidable as might be supposed, if *Medians* instead of Arithmetic Means be employed. In view of abnormalities like the irregular rise of prices in 1873, there would be a peculiar propriety in the use of the Median.[1]

Exactly similar considerations apply to the factors or proportions which form Mr. Giffen's second table. It was open to him, as he points out, to take these proportions from some other year than 1875. In fact, he tried several years with substantially identical results.[2] There are, therefore, at once as many factors as there are years in the series. Moreover any mean of these factors may be taken. Here, again, then, we have $2^n - 1$ schemes to choose from.

Well, then, any one of these $(2^n - 1)$ measuring-rods may be used in connection with any one of the $(2^n - 1)$ price-scales above mentioned. Thus arise $(2^n - 1)(2^n - 1)$ arrangements for comparing the years in respect of the level of prices. To these may be added Mr. Palgrave's system of factors combined with any one of the $(2^n - 1)$ price-scales. This addition swells the contingent to $2^n \times (2^n - 1)$. This number is to be added to the previous estimate, viz., $2^n \times (2^n - 1)$, which is thereby doubled, becoming $2^{n+1}(2^n - 1)$.

The question may now arise, How large is $n$ to be? It may be suggested that it should be as small as possible, namely, 2. We should proceed according to the method recommended by Professor Marshall,[3] and exhibited at length in the former Memorandum.[4] We should compare the present year with last year only, next year with the present, and so on. The fact that Professor Marshall refers to the general problem of a measure based on articles of consumption, whereas we are now particularly concerned with the volume of trade, does not appear to affect the reasons on which his recommendation is based. However, it may be well to combine that principle with the practice of averages over several years. At any rate, the latter procedure is countenanced by the most eminent statisticians. Extending their review over a considerable tract of time, they have, in effect, taken for granted that sort of solidarity between the years which we have all along supposed. Ten years, twenty years, nay, even forty years, have thus been compared *inter se*. Let us take the

[1] See below, Section V., and the papers to which reference is there made.

[2] *Parl. Papers*, 1878–9, C. 2247.

[3] *Contemporary Review*, March 1887. [4] *Brit. Assoc. Report*, 1887, p. 269.

period of twenty years as quite permissible; then by the formula above reached we find the total number of available arrangements to be more than a *billion*.* All these billion schemes are on the whole about equally good, some having a slight advantage in respect of safety, and others of ease.

## SECTION IV.

### *Mr. Bourne's Method.*

These elucidations assist us in discerning the character of a method which was proposed by Mr. Bourne so long ago as 1873,** and more recently has been submitted by him to the British Association together with some criticism of Mr. Giffen's celebrated computations.[1] It will be found that Mr. Bourne has discovered, not *the* method, but only *a* method—a very good method, no doubt, but not much better than many others, not more serviceable than hundreds, not more accurate than millions that are available.

A little attention will show that Mr. Bourne's reasoning is virtually identical with that which Mr. Giffen employs in his fourth table when he compares the quantities in any year at the prices of that year with the same quantities at the prices of 1883; and goes on, as, for instance, in his first Report, page v, to compare the measure (for the level of prices) so obtained in order to deduce the comparative volume of any year from its value. It is not to be denied, indeed, that this method, under the neat handling of Mr. Bourne, has acquired great elegance. But we must take care not to exaggerate its pre-eminence over other methods.

In the first place it does not seem to have any advantage over the twin-method which was noticed along with it above. This method is, in brief, to take as the measure of changed level of prices

$$\frac{\text{Quantities of 1883 at prices 1887}}{\text{Quantities of 1883 at prices 1883}}.$$

There is no reason to think that this method would be less accurate than its converse. And it would enjoy the distinction of not having been worked out in detail by Mr. Giffen (in his latter tables).

* Compare Irving Fisher's *Making of Index-numbers*.

** The fact that the priority in the method of calculating the volume of trade belongs to Bourne may justify the attention here given to his opinions.

[1] *Brit. Assoc. Report*, 1885 and 1888.

A certain precedence, perhaps, attaches to these twin-methods in virtue of a slight superiority in ease and elegance.[1] But this slight distinction must not be mistaken for a serious difference in worth or power. Nor is Mr. Bourne's position defensible when he disapproves the method set forth in Mr. Giffen's first three tables. The gist of Mr. Bourne's objections is contained in the following passage, of which the context should be studied :—[2]

"The proportions of [quantities of] cotton yarn for 1865, 1875, 1883 stood as 104 : 216 : 265, but by value as 10 : 13 : 14, and the percentages of increase or decrease from the standard of 1861 were as + 91·23 : + 16·91 [misprinted in the Report 41·63] : — 2·3. It is difficult to see how any combination of these factors, so widely differing in their ratios, can bring about the result that the index-numbers for cotton yarn should be altered as + 5·38 : + 1·00 : — 0·14 as shown in the Board of Trade tables."

This passage, with its context, presents great difficulties. As Mr. Giffen's "index-numbers" do not purport to be measures of volume, but of changed level of prices, there is no reason for surprise that the "factors" of quantity and value should have no visible effect on the "result that the index-numbers for cotton yarn should be altered" by certain additions. The additions to the index-number are proportional to the percentages of increase or decrease of price (+ 5·38, + 1, — 0·14; proportional to + 91·23, + 16·91, — 2·31), and that is all that is to be expected. It seems as if the original writer had stated the relation between a yard and a metre as a preliminary to comparing the height of an Englishman and a Frenchman, the former height having been given in yards, the latter in metres. The critic gives the relative height of the Englishman and Frenchman, and then complains that this factor has no correspondence with the relation between a yard and a metre.

Such appears at first sight to be the drift of the passage above cited. It will be found, however, from the context that the critic has not overlooked the fact that the object of the "index-number" in question is, to continue our metaphor, the comparison of the two scales, yard and metre. But he seems under the mistaken impression that this comparison can best be effected by giving the Frenchman's height in metres and also in feet, and comparing these figures. Now, it is here contended that the two scales may equally well be compared by taking the Englishman's height

[1] Above, pp. 231, 273.

[2] *Brit. Assoc. Report*, 1885, p. 868.

both in metres and feet.[1] Nay, a German will do equally well for the purpose of comparing the two scales of measurement.[2] But, in order to bring out the truth which is here implied, it will be well to employ a metaphor which is more nearly an analogy.

The following apologue may put the whole matter in a clear light. Suppose there were given the increase per cent. in the number of births in a certain district, the increase per cent. in the number of the population, and in the number of persons to a birth (or the inverse birth-rate) for several years. There would, of course, be a visible connection between these figures; and any one set, in particular the proportionate population, could be deduced from the other two. Now, if a statistician had assigned an index-number purporting to represent the alteration in the numbers of the population, and the alterations so assigned were not deducible from the first and third sets of data, and not coincident with the second, it would, no doubt, be reasonable to complain that it was difficult to see how the given factors brought about that result.

But our problem is by no means so simple. It is like those problems in vital statistics which Laplace, in the absence of a complete census, proposed to solve by the aid of the Calculus of Probabilities. He supposes that the total number of births in a country has been ascertained from registers of baptisms, and that the birth-rate, or its reciprocal, the number of persons to one birth, has been observed at two or more epochs in several districts, which are taken as fairly representative of the whole country. If the birth-rate were constant from year to year, we might reason thus :—

Population in year $y$ : Population in year $x$ :: Total No. of births in $y$
: Total No. of births in $x$ ($x$ being the standard year).

But if the birth-rate is considered as varying between the two epochs compared a correction must be made for this circumstance. We have then :—

$$\text{Population in } y = \text{population in } x \times \frac{\text{Total No. of births in } y}{\text{Total No. of births in } x} \div \frac{\text{Average birth-rate in } y^1}{\text{Average birth-rate in } x}.$$

Now, the last-written fraction may on certain suppositions

[1] The twin-method alluded to on our page 278.

[2] Mr. Giffen's first method.

be determined by taking a measure of the variations in birth-rate (at one epoch compared with another) in each of the observed districts, with due attention to the varying size of the districts, the different importance (for the purpose in hand) of these rates. In other words, if the districts are named $a$, $b$, etc., we may write.

$$\frac{\text{Average birth-rate in } y}{\text{Average birth-rate in } x}$$

$$= \frac{\text{Population of } a \text{ in } y \times \text{birth-rate of } a \text{ in } y + \text{population of } b \text{ in } y \times \text{birth-rate of } b \text{ in } y + \text{etc.}}{\text{Population of } a \text{ in } y \times \text{birth-rate of } a \text{ in } x + \text{population of } b \text{ in } y \times \text{birth-rate of } b \text{ in } x + \text{etc.}}$$ [1]

This is the analogue of Mr. Bourne's method, in which it will be seen that *there is postulated a certain constancy in the proportions, between the population of each district, to that of the others and of the whole country.*

Suppose a writer had employed the proportions furnished by the year 1883 in order to determine the relation between the birth-rate of that year and of the year 1865.[2] He, in effect, postulates the constancy of proportions (between the different districts and the whole country) to prevail over that period. It is not open, then, to him to complain of another writer who employs the proportions furnished by the year 1875 in order to compare the population for a series of years between 1865 and 1883. But, if the use of those proportions is admissible, then the sort of verification which the writer of the vexed passage under review appears to expect was not to be expected.

In short, given the hypothesis which has been hinted metaphorically here, and stated explicitly above, the method which Mr. Bourne has propounded has no great advantage over the other methods. That hypothesis not being given, Mr. Bourne's method, equally with the others, falls. Of the varied ramifications of the problem he has occupied a particular, and no doubt an eminent, branch. He cannot hope that this particular branch should stand when the others have fallen. One can only bring them down by striking at the root of the whole reasoning.

From this class of methods we shall now proceed to a substitute for them, which has recently been proposed by Sir Rawson Rawson.

[1] Compare the general formulæ given above, p. 275.

[2] Cf. *Brit. Assoc. Report,* 1885, p. 865 *et seq.*

## SECTION V.

### *Sir Rawson Rawson's Method.*

Sir Rawson Rawson's original method may be contemplated under two aspects, according as the primary object is to measure variations in the volume of trade or—our peculiar care—in the value of the monetary standard. Sir Rawson's solution of the problem in its former phase is simple: to put the tonnage of "ships cleared or entered with cargoes"[1] as representing the volume of exports and imports.

Now, we have seen above that volume of trade must be understood in some such sense as equivalent, or rather proportional, to volume of value estimated in a corrected monetary standard, or, if the expression is not too harsh, volume of utility as measured by money. Therefore, in order that the new method should be available for the comparison of volumes in different years, say $x$ and $y$, the following equation ought to hold approximately:—

$$\frac{\text{Tonnage in year } y}{\text{Tonnage in year } x} = \frac{\text{Corrected value in year } y}{\text{Corrected value in year } x},$$

where "corrected value" is used as a short title for the figure which is obtained by reducing the total value for each year to a standard or normal level. In other words,

$$\frac{\text{Tonnage in year } y}{\text{Tonnage in year } x}$$

$$= \frac{\text{Quantity of } a \text{ in } y \times \text{normal price of } a + \text{quantity of } b \text{ in } y \times \text{normal price of } b + \text{etc.}}{\text{Quantity of } a \text{ in } x \times \text{normal price of } a + \text{quantity of } b \text{ in } x \times \text{normal price of } b + \text{etc.}}$$

Now, "tonnage" is the measure of a ship's capacity for cargo. Tonnage is, or is proportioned to, the cubical capacity of that part of a ship which is available for cargo.[2] Accordingly

[1] Given in the *Statistical Abstract.*

[2] Accordingly Sir Rawson Rawson's priority is not affected by Drobisch's suggestion (noticed in the former Memorandum) to put the number of tons or hundredweights in the total mass of commodities as the measure of their volume. *Mutatis mutandis*, the tests here applied to Sir Rawson's method are applicable to that of Drobisch. The validity of the latter is confirmed by the statistics of the German foreign trade for 1885 and 1886, which have recently been published by the Board of Trade, along with an estimate of the change in volume between 1885 and 1886, based upon the method employed in Mr. Giffen's Table IV. (*Parl. Papers*, 1888, C. 5597). The "quantities" of the German exports and imports are all expressed in (German) tons, so that Drobisch's method is readily applicable. The following tables exhibit the results of that method in contrast with the theoretically more perfect computation. The results are expressed as index-numbers for the volume and the level of prices in 1886 as compared

the first step towards establishing the relation above stated is to show that the capacity for cargo bears from year to year a constant ratio to the space actually occupied by cargo. In other words, we require to be assured that an average ship (entering or clearing with cargo) is as fully loaded in one year as another. Sir Rawson Rawson, whose sagacity and candour have anticipated every objection, is satisfied that we may dismiss this scruple.

We may therefore write the postulated equation :—

$$\frac{\text{Bulk of } a \text{ in } y + \text{bulk of } b \text{ in } y + \text{etc.}}{\text{Bulk of } a \text{ in } x + \text{bulk of } b \text{ in } x + \text{etc.}}$$

$$= \frac{\text{Quantity of } a \text{ in } y \times \text{normal price of } a + \text{etc.}}{\text{Quantity of } a \text{ in } x \times \text{normal price of } a + \text{etc.}}$$

where " bulk of $a$ in $y$ " is short for the total space, the volume in cubic yards, occupied by the whole mass of commodity $a$ which is exported, or as the case may be imported, in the course of the year $y$. The relation of these two fractions may be better seen by putting each of them in the form of what may be called

with 1885. The imports and exports of the precious metals have not been included in the data :—

| German Imports of 1886 comparative with those of 1885. | Drobisch's Method. | Mr. Giffen's Method. |
|---|---|---|
| Index-number for Volume . . . | ·95 | ·98 |
| Index-number for Price-level . . | 1·03 | ·99 |

| German Exports of 1886 comparative with those of 1885. | Drobisch's Method. | Mr. Giffen's Method. |
|---|---|---|
| Index-number for Volume . . . | 1·01 | 1·04 |
| Index-number for Price-level. . . | 1·04 | ·976 |

This complete consilience affords an indirect verification of Sir Rawson Rawson's method, in so far as it is on the same footing with Drobisch's; each admitting of being regarded as an arbitrarily weighted mean of certain ratios, such as

$$\frac{\text{tons of commodity } a \text{ in } 1886}{\text{tons of same commodity in } 1885}$$

(the ratios of quantity described above). Whereas the theoretically correct expression is the value of commodity $a$ at normal (or corrected) prices; Drobisch puts tons avoirdupois of $a$, and Sir Rawson Rawson puts (in effect) tonnage (or cubical volume) of $a$.

From this point of view it will appear that both methods derive confirmation from the experiment tried above, at p. 266, of taking an altogether unweighted mean of the ratios between quantities.

a "weighted mean" of the ratios of bulk; (or, as implied in the last note, we might take as the ratios to be operated on: $\frac{\text{Quantity (in tons or gallons) of } a \text{ in } y}{\text{Quantity of } a \text{ in } x}$, etc.). To effect this in the left-hand member of the equation, we should leave the denominator as it is, and we should alter each term of the numerator thus: For Bulk of $a$ in $y$ write Bulk of $a$ in $x \times$ $\frac{\text{Bulk of } a \text{ in } y}{\text{Bulk of } a \text{ in } x}$, and so on. The left-hand side of the equation is now in the form of a weighted mean of the ratios, $\frac{\text{Bulk of } a \text{ in } y}{\text{Bulk of } a \text{ in } x}$, etc., the weights being bulk of $a$ in $x$, bulk of $b$ in $x$, etc. Treating the right-hand member in the same spirit, we obtain a weighted mean of the same ratios, each weight being of the form, Bulk of $a$ in $x \times$ No. of tons [gallons, pieces, etc.] in unit of bulk $\times$ normal price of ton [gallon, piece, etc.], or, as it may be more shortly written, value of Bulk of $a$ in $x$ at standard prices—that is, assuming that the number of tons, etc., in a unit of bulk is constant from year to year. But if this cannot be assumed we must add a remainder, of which the numerator is made up of terms like the following:—

Bulk of $a$ in $y$ (No. of tons in unit bulk of $a$ in $y$ — No. of tons in unit bulk of $a$ in $x$) $\times$ normal price of $a$; and the denominator is the total value in $x$.

Omitting this remainder for the present we have now to compare two weighted means of the same set of quantities (the ratios above specified), the weights being in the one expression each of the form, bulk of $a$ in $x$; in the other expression of the form, value of $a$ in $x$. Now, it has been shown by the present writer in a Memorandum on the Accuracy of Index-numbers, published in the Report of the British Association for 1888, that, in forming a mean of any given set of quantities, the difference between the results obtained by adopting different systems of weights is apt to be inconsiderable. This proposition has been established both by reasoning from the theory of probabilities and by pretty copious examples. It is shown that the divergence between the two results tends to diminish as the number of (supposed independent) items entering into the average increases; the probable deviation being proportioned to the inverse square root of the number of items. This tendency to evanescence is resisted by three circumstances: the inequality of the given items which are to be averaged, the inequality of the weights which con-

stitute the set or system which is regarded as true, and the largeness of the difference between each weight in that one system and the corresponding weight in the other system. It can be shown that the last two circumstances are equivalent to, or, rather, are contained under, one attribute, namely, the inequality of the weights in either system.[1]

These criteria are now to be applied to the case before us. In the first place we have a very large number of elements to deal with—much larger than the number of enumerated articles which enter into Mr. Giffen's index-numbers. For Sir Rawson Rawson's index-number includes the unenumerated as well as the specified articles. There is, therefore, a strong *prima facie* presumption that the divergence between the two compared expressions will prove to be unimportant; even smaller than in the case of the index-numbers compared in the paper referred to, the number of items being larger here than there.

Then, as to the counter tendencies. There is no reason to apprehend any fatal inequality in the ratios of the form $\frac{\text{Bulk of } a \text{ in } y}{\text{Bulk of } a \text{ in } x}$. At least it would only be in cases of articles where the bulks were very small that such an influence need be apprehended, the ratio in such a case tending to infinity. It is easy to see, however, that this tendency would be corrected by the "weights"; that such an article would not be likely to have much effect on the whole expression. There seems no reason to apprehend any much more marked inequality in comparative bulks than in comparative quantities, which, as we know from Mr. Giffen's tables, are not fatally unequal.

There remains the twofold condition that the weights of either system should not *inter se* be very unequal. The most serious violation of this condition seems to be coal in the case of exports. It appears from Sir Rawson Rawson's statistics that the bulk of coal takes up an inordinate proportion of the total bulk of all commodities. Accordingly he has very properly excluded coal from his index-number. It is interesting to observe that, as shown in Tables I. and VII., the inclusion of coal does not, as a matter of fact, distort the result so much as might

[1] The measure of "inequality" is the square root of the sum of squares of all the weights in a system ÷ their sum. The divergence between the results is directly proportionate to this expression. If the weights are perfectly equal the factor reduces to *unity* $\div \sqrt{n}$. But suppose one weight preponderates over its fellows to such an extent as to constitute half of the total mass, the remainder of which we may imagine split up among a number of small weights; the resulting expression is no longer of the order $1 \div \sqrt{n}$, but equals, at least, $\frac{1}{2}$.

have been expected, or indeed in any considerable degree. The tables referred to should be compared with the Appendix at p. 336, as strikingly illustrating how different principles of averaging bring out the same mean result; in short, that in our sort of work it is not very easy to go wrong.

Among imports, grain and timber are suspicious. But with regard to timber Sir Rawson Rawson shows that, though the "weight" (determined by its bulk) is large, yet it is not materially different from what it ought to be as determined by value. However, he is no doubt judicious in excluding such-like items from his final index-number.

With regard to the inequality of *values*, as this has not proved fatal to Mr. Giffen's and the cognate methods, there is *a fortiori* less reason to apprehend it in the case of an argument which is based on a greater number of independent items. However, it might be well to examine specially the influence of cotton.

There remains to be considered the remainder, which is made up of differences between the density of packing in different years. It is natural to suppose that these should compensate each other except so far as in the course of years a general tendency to increased economy of room makes itself felt. Sir Rawson Rawson sets off against this tendency the increase of passenger traffic; a quantity which he has abundantly shown to be of an order which may be neglected. For short periods, at any rate, the new method appears to constitute an important adjunct to, if not a complete substitute for, the received methods.

Sir Rawson Rawson's method may be regarded in another aspect as affording a measure of the level of prices in different years. If the hypotheses made in the earlier part of this paper are conceded, no additional remark is called for here. We have simply to write Index-number for level of prices in year $y$ as compared with $x$ = average price in $y$ ÷ average price in $x$ = $\frac{\text{Value in } y}{\text{Volume in } y} \div \frac{\text{Value in } x}{\text{Volume in } x} \left(\text{or } \frac{\text{Value in } y}{\text{Value in } x} \div \frac{\text{Volume in } y}{\text{Volume in } x}\right)$; where the values are given figures and the volumes are proportioned to the respective tonnages. We thus obtain a new and remarkably easy solution of our problem.

## Section VI.

### *The present Writer's Method.*

We have so far (in this third Memorandum) been supposing that the importance attached to each variation in price is,

or ought to be, proportioned to the value of the corresponding article. But we have now to entertain a different supposition and distinct method. We are now to imagine a general change coming over the monetary world—or some zone of it like wholesale prices—like a general variation in temperature or atmospheric pressure over a physical region which is not perfectly level and uniform in its conditions. In reading a barometer or thermometer in any particular place with a view of ascertaining the fact and amount of a general change it would not be appropriate to attach importance to the mere size of the tube and quantity of the rising or falling liquid. In fact the *smaller* thermometer has so far the preference, as it takes on more quickly changes of temperature in the surrounding medium. Sensitiveness, not size, is the criterion of these indicators. So also, in virtue of well-known analogies between the unity of price in the same market and the equilibrium of fluids in the same vessel, the change of price in a large market is not more indicative of the sought mean variation than a change of price in a small market. *Prima facie*, for the purpose in hand, each observation should count for one. Or, if more weight attaches to a change of price in one article rather than another, it is not on account of the importance of that article to the consumer or to the shopkeeper, but on account of its importance to the calculator of probabilities, as affording an observation which is peculiarly likely to be correct—peculiarly likely to coincide with that *type* which he is seeking to elicit.

This type of mean variation may be generally defined as that figure which would be presented most frequently if we were to continue indefinitely the long series of price-ratios, or at least that return in whose neighbourhood the greatest number of these statistics cluster. It is, in other words, the Greatest Ordinate of the complete curve, or the highest column of the rectilinear diagram, which represents by its abscissa ratio between the prices of two compared epochs, and by its ordinate the frequency with which that ratio would be returned if the statistics were extended over every region of industry which is subject to independent fluctuations. It is even allowable to imagine series of statistics still longer,[1] namely, those which would ideally occur if we could go on and on multiplying observations under unchanged conditions. As Dr. Venn says :—

"We say that a certain proportion begins to prevail among

[1] Compare Dr. Venn, *Logic of Chance*, chap. i. § 14.

the events in the long run; but then, on looking closer at the facts, we find that we have to express ourselves hypothetically, and to say that, if present circumstances remain as they are, the long run will show its characteristics without disturbance."

The grounds for thus defining our *quæsitum* were stated in that part of the former paper which referred to semi-objective averages or types. There should be added a reference to the sections on the Greatest Ordinate in Dr. Venn's *Logic of Chance.*[1] Compare also the following weighty words in the masterly study on "Cambridge Anthropometry" which he has recently contributed to the Anthropological Institute: "The ordinary mean here is obviously an imperfect guide. . . . What we ought to do, owing to the obvious asymmetry of the curve of frequency, is to take, not the arithmetic mean, but what is called 'the point of maximum frequency,' as this is a far truer index of what may be considered the normal length of vision." Dr. Venn is discussing a problem analogous to ours, namely, how to extricate from an *unsymmetrical* group of observations that mean value which may be taken as a representative type.*

Such being the question, it might seem appropriate to put as answer that return which occurs most frequently in the statistics actually given. But it must ever be remembered, though it is often forgotten by statisticians, that the statistics of prices with which we have to do are of the nature of *samples:* specimens taken at random from a much larger, if not an indefinitely large series. In interpreting these evidences, in inferring the type from a limited number of individuals, we must be guided by the methodical rules which the Calculus of Probabilities prescribes. The theory of errors of observation is here as high above ordinary induction as in the general field of modern science the received inductive methods transcend the simple enumeration of the ancients. Now, the Calculus of Probabilities teaches that the best answer to our question will

[1] The third edition of this unique work, especially the first two chapters and the last two chapters, should be studied by all who wish to contemplate that phase of our problem which is now under consideration.

* The statistics with which Venn was dealing were imperfect, one extremity of the frequency curve not being given; otherwise it may be doubted whether the preference expressed in our text for the greatest ordinate is in general justifiable for a purpose like that of Venn, which was to determine whether the average eyesight of Cambridge "honour-men" differed significantly from that of "poll-men." For—in addition to the difficulties acknowledged in our text—the Mode labours under the disadvantage that there is not available a measure of its accuracy, it does not afford a test of significant difference.

not be obtained by taking that which on the face of the evidence seems to be manifested.[1]

The need of this caution is illustrated by the annexed statistics. Looking at these three groups of statistics you might conclude that the first one, designated A, emanated from and, if prolonged, would converge to 30, as that number is the one most frequently repeated. It might similarly be inferred that B in the same sense represents 38. With regard to C, there might be more hesitation, since no one place or figure preponderates. If, however, we double the size of our compartments and consider which is the fullest of these enlarged places, that distinction will be found to belong to 57–58. Accordingly 57·5 might seem the type represented by this group.

But in fact all these groups appertain to the same series, each figure in all of them being formed in the same way, namely, by the addition of ten digits taken at random from mathematical tables. If this series were indefinitely prolonged the figure most frequently repeated would be 45, a figure which in two of the groups does not even occur once. A much better approximation to the greatest ordinate of the complete series is obtained by taking an average other than the greatest ordinate of each set of samples. For instance, the *median*—or figure which has as many of the given observations above it as below it—is for A 42, that being the fourteenth figure in the group of twenty-seven. Similarly the median of B is 44; of C 50. The median of the whole set, numbering eighty-one, is 45; whereas the greatest ordinate is *prima facie* 38, or perhaps 57·5.[2]

[1] Well does Dr. Venn say in the context of the passage cited from his *Cambridge Anthropometry:* "Any successful appeal to this [the point of maximum frequency] requires far more extended statistics than those at our disposal." Yet he has 520 returns before him!

[2] See *Journal Royal Statistical Society*, June 1888, where it is attempted to meet the difficulty presented by such ambiguity. The method there recommended is to rearrange the statistics in larger groups defined by a new "degree" or "unit" which is some multiple of the given one (that is, of unity in our example). The unit to be adopted is the *smallest* interval which will bring out the one-headed character of the curve; in the cases above instanced generally 6 or 7. Now, we may begin this operation not only from either extremity of the given discontinuous curve (as stated in the paper referred to), but also with equal plausibility from any intermediate point. There are thus about as many systems as the new degree is greater than the old one; in the cases before us usually six or seven. The apex of *any* of these arrangements giving an equally plausible solution, it is proper to take the Mean of them all. I have performed this operation on each batch of twenty-seven figures (given in the text), and on the united eighty-one, with results in each case differing very little from the Arithmetic Mean, which is the best answer that can be extracted from these data.

Professor Unwin, to whom this problem has been submitted, recommends

It appears, then, that, though our end is the greatest ordinate of the complete series, the best *mean*—if we may be excused a pun which it is not easy to avoid—is not necessarily the greatest ordinate of the sample group. The position of greatest frequency is an object, like happiness, best reached by not aiming at it too directly.

The indirect and ancillary average need not be the one which we have taken for the sake of illustration in the last paragraph. In fact, in the case there instanced the arithmetic mean would be the preferable method. But in the case of prices there is reason to believe that the median is peculiarly appropriate. The nature and varieties of this mean have been fully discussed by the present writer both in his former Memorandum on the same subject as the present one, and also in the Memorandum of 1888 On the Accuracy of Index-numbers.

However, it may not be out of place here to give an additional example taken from the statistics of exports. In the annexed table each figure in the first column (on the left hand) expresses a proportion between the price of an article in 1887 and the price of the same article in 1883. Thus, the price of gunpowder per lb. being in 1887 6·46*d.* and in 1883 5·83*d.*, we have the proportion, or comparative price, $111 = 100 \times 6{\cdot}46 - 5{\cdot}83$.

Opposite each comparative price are written in the second column or space the values of the corresponding articles for 1887, the proportionate values or actual values divided by a certain figure which is the same for all the entries, viz. 240,000. For

forming a derived curve by joining the tops of each pair of adjacent ordinates in the given discontinuous curve; and continuing this process of graphical derivation until we reach a smooth (one-headed) curve. He has been so kind as to subject to this treatment the eighty-one figures above given, and after *eight* repetitions of the process finds for the eighth derived curve one whose greatest ordinate is 43—a very respectable approximation, when we consider that what may be called the real point is 45; that the result given by the Arithmetic Mean, which is here the best solution, is 45·2; and that the probable error to which even that best solution is liable is 1·4.

These processes are, however, very troublesome. Still, in doubtful cases, it may be well to check the Median by recurring to first principles and ascertaining the whereabouts at least of the Greatest Ordinate.

| | |
|---|---|
| A | 27 30 31 32 33 34 36 37 40 41 42 43 46 47 48 49 50 51 52 59 64 |
| | 30 41 42 46 52 |
| | 30 |
| B | 29 31 32 33 34 36 38 39 41 43 44 45 46 49 50 52 53 56 57 59 62 |
| | 38 45 46 |
| | 38 45 |
| | 38 |
| C | 38 40 41 42 44 46 47 49 50 51 52 54 57 58 61 |
| | 40 41 44 46 50 57 58 |
| | 40 50 57 58 |

instance, the value of gunpowder is 1, being, in round numbers, its actual value 260,000 divided by 240,000. With the reason for adopting this divisor we are not here concerned. Any other basis would serve our purpose as well. It often happens that the same proportion of price is enjoyed by two articles. Thus the comparative price 127 appertains both to arms (fire) and to silk, of which articles the proportionate values are respectively 1 and 6. Accordingly against the entry 127 are written (it does not matter in what order) the figures 1 and 6. Both the prices and the proportions of value are taken from the table given by Mr. Bourne in the paper on index-numbers contributed by him to the Report of the British Association for 1888.

Well, then, the simple or unweighted median is thus found. There being in all 64 proportions (some of them coincident), we are to select that one which has as many returns above it as below; in short, a point between the thirty-second and thirty-third in the order of magnitude. This is easily effected by counting up the numbers of the "proportionate values" in the right-hand space. The thirty-second and thirty-third, counting from the highest, are 12 and 4, both corresponding to the ratio 89. The simple median is thus 89.

| | | | |
|---|---|---|---|
| 127 | 1, 6 | 92 | 7, 5 |
| | | 91 | 11 |
| | | 90 | 42, 2, 5, 3, 3,[2] 7 |
| | | 89 | 47, 12, 4 |
| | | 88 | 79, 4 |
| | | 87 | 137, 16, 2 |
| | | 86 | 5 |
| | | 85 | 17 |
| 113 | 4 | 84 | 4, 1 |
| 112 | 2 | 83 | 29 |
| 111 | 1 | 82 | 3 |
| 110 | | 81 | 1, 6 |
| 109 | | 80 | 1 |
| 108 | 1 | 79 | 3 |
| 107 | | 78 | 8, 1, 6, 3 |
| 106 | | 77 | 11,[2] 20 |
| 105 | | 76 | 1 |
| 104 | 2, 41 | 75 | |
| 103 | | 74 | |
| 102 | 2 | 73 | 19 |
| 101 | 17 | 72 | |
| 100 | 2,[1] 1, 2 | 71 | |
| 99 | | 70 | 1, 5, 9,[2] 2, 3 |
| 98 | 2 | 69 | |
| 97 | | 68 | |
| 96 | 7, 7, 4 | 67 | 4 |
| 95 | 1 | 66 | 5 |
| 94 | 0, 6, 1 | 62 | 2 |
| 93 | | | |

[1] Comparative price of *stockings* per dozen; not explicitly given by Mr. Bourne but inferable from the entries in his *value* and *volume* columns.

[2] Not explicitly given by Mr. Bourne, but inferable from his data.

It was pointed out in the former Memorandum that there is a plausible hypothesis on which, even for the present purpose, it is proper to attach some importance to the values of the commodities, though not necessarily that degree of importance which is prescribed for the standard based on national consumption. The simplest method of attaching importance to the values is to take the simple median of the ratios on the supposition that each of them occurs as often as the number which indicates the corresponding value, or the sum of such numbers where there are more than one of them. Upon this understanding there are in all 666 constructive observations—as near as may be, half above and half below 88. That figure then is the weighted median.

It is pretty certain that this complex median assigns too much importance to the values. And it is probable that the simple median assigns too little. Accordingly a good solution is afforded by combining or comparing the two results, in the example before us taking 88·5 for the answer. Should the two results be markedly different, inquiry may be made as to the cause of the difference, and a preference should be given in general to the simpler combination.

A more elaborate method of weighting the median by taking the square roots of the values was recommended in the former Memorandum. But on second thoughts it appears that the special advantages which this plan may confer hardly compensate for the additional trouble which it involves.

For further illustrations and suggestions the reader is referred to the writer's paper, "On some New Methods of Ascertaining Variation in General Prices," in the *Journal of the Royal Statistical Society* for June 1888. It is hoped that the familiarity of the arithmetic mean will not prevent statisticians from attending to the reasons for preferring in certain circumstances the Median.

## SECTION VII.

### *Ricardo's Method.*

Ricardo suggests a method of measuring variation in the value of money, when he lays down that a commodity "which at all times requires the same sacrifices of toil and labour to produce it" is invariable in value.[1] From this point of view the Labour Standard is to be regarded as independent and substantive, not subsidiary to the "Consumption" (or any other) standard, as represented in the first report of the Committee. The Labour

[1] *Principles*, III. chapter xx. (On Value and Riches).

Standard thus conceived and the Consumption Standard are to each other as "value" and "riches" in Ricardo's terminology. "The labour of a million of men in manufactures will always produce the same value, but will not always produce the same riches. . . . A million of men may produce double or treble the amount of riches of 'necessaries, conveniences, and amusements,' in one state of society that they could produce in another, but they will not on that account add anything to value."[1] The Consumption Standard measures the change of money with respect to "riches"; the Labour Standard with respect to "real value." The former relates to the utility of consumption; the latter to the disutility of toil.

Ricardo only proposes the idea of an invariable commodity, of which "we have no knowledge, but may hypothetically argue and speak[2] about it as if we had." He does not assist us to ascertain the change in the pecuniary worth of that hypothetical commodity. A more definite scheme is suggested by the remarkable passage of Professor Marshall's evidence before the Royal Commission on Gold and Silver, where he says, speaking of appreciation of gold: "When it is used as denoting a rise in the real value of gold, I then regard it as measured by the increase* in the power which gold has of purchasing labour of all kinds—that is, not only manual labour, but the labour of business men and all others engaged in industry of any kind."

It may be remarked on this that the Labour Standard and the Consumption Standard present a certain analogy, the former standing in much the same relation to the fundamental laws of Supply as the latter to those of Demand. As before we posited as normal certain quantities of purchasable commodities, and compared the pecuniary worth at different epochs of that constant sum of commodities; so now we should posit certain amounts of work of various sorts, and compare the pecuniary remuneration required at different epochs for the same quantity of work. Or, in other words, we should form the ratio of "new" to "old" rate of pay in each department of industry, and take the mean of this set of ratios, each *weighted* by the amount usually paid in the corresponding department.

[1] *Principles*, III. chapter xx. (On Value and Riches).

[2] "And," he adds, in the exclusive spirit which has characterised almost every propounder of an original method, "may improve our knowledge of the science, by showing distinctly the absolute inapplicability of all the standards which have been hitherto adopted."

* The word "increase" has here been substituted for the obviously misprinted "diminution" in the original Report (Question 9625).

Moreover, since upon Ricardian principles the value in exchange of commodities is proportioned to the "comparative quantity of labour expended on each," there may be expected some correspondence between the two expressions, not only as to their general form, but also as to the constants which they involve, the *weights* with which the variations of wages and prices are respectively to be affected. But the idea of such a correspondence is marred by the fact that the denominations of finished products do not coincide with the classification of wages. Also the suggested analogy is vitiated by a circumstance which is of great theoretical importance: that values in exchange—and accordingly the proportions which form the weights of the Consumption Standard—depend not only on quantity of labour, but also on interest, according to the different degrees of durability of the capital employed in producing them. This circumstance, as it creates a difficulty [1] with regard to Ricardo's first principles, so it suggests a scruple about the method which is here connected with those principles. When we "hypothetically argue and speak" of an invariable commodity "which at all times requires the same sacrifice of toil and labour to produce it," [2] should we include in the idea of "sacrifice" not only bodily an[illegible] labour, but also *abstinence*?* Shall we introduce into [illegible] in th[illegible]ber the variation in the rate of Interest, weighted [illegible] amount paid in the way of Interest? Or shall we [illegible] example of the great theorist himself, and omit the consideration of Interest as often as convenience and rotundity of statement and the purpose of a rough approximation may require? The management of these and other difficulties connected with the Labour Standard must be resigned to the abler hand which has already touched this part of the subject.

[1] *Cp.* Sidgwick, *Political Economy,* Book I. ch. ii. "It is rather a perplexing question how Ricardo and M'Culloch could deliberately adhere to the statements above quoted [that labour is the measure of the real value of things, etc.], while they at the same time drew attention to the differences in the value of different products, due to the different degrees of durability of the capital employed in producing them."

[2] Ricardo, *loc. cit.*

* I should now answer to this question "yes;" having regard to Marshall's above cited definition of "appreciation," with which may be compared the conception introduced in his later work with reference to international trade, of representative "bales," "each of which represents uniform aggregate investments of her [country's] labour (of various qualities) and of her capital." I have altered some passages in the context, so as to exclude the term "wages" which, designating a *share* in the total product, is not germane to the Ricardo-Marshall conception.

## CONCLUSION.

In conclusion it may be useful to enumerate and summarily characterise the principal definitions of the problem, or "Standards,"[1] which have been discussed in this and the preceding Memorandum. An alphabetical order will be adopted, the order of merit being not only invidious, but also impossible in so far as different methods are the best for different purposes.

1. The *Capital Standard* takes for the measure of appreciation or depreciation the change in the monetary value of a certain set of articles. This set of articles consists of all purchasable things in existence in the community, either at the earlier epoch or at the later epoch, or some mean between those sets. This standard is due to Professor Nicholson. It is stated by him (in terms a little less general than those here adopted) in his book on *Money*. It is discussed in the sixth and the tenth sections of the former Memorandum.

2. The *Consumption Standard* takes for the measure of appreciation or depreciation the change in the monetary value on a certain set of articles. This set of articles consists of all the commodities consumed yearly by the community either at the earlier or the later epoch, or some mean between those two sets. This standard has been recommended by many eminent writers, in particular by Professor Marshall in the *Contemporary Review* of 1887. It is proposed by the Committee as the principal standard. It is discussed in the second section of the former Memorandum.

3. The *Currency Standard* takes as the measure of appreciation or depreciation the change in the monetary value which changes hands in a certain set of sales. These sales comprise all the commodities bought and sold yearly at the earlier epoch or at the later epoch, or some mean between those quantities. This standard appears to be implicit in much that has been written on the subject, but to have been most clearly stated by Professor Foxwell. It is discussed in the second section of this Memorandum.

4. The *Income Standard* takes as the measure of the appreciation or depreciation the change in the monetary value of the average consumption, or in the income per head, of the community. This standard is proposed in the fourth section of the former Memorandum.

[1] The methods discussed in connection with the names of Mr. Giffen, Mr. Bourne, and Sir Rawson Rawson are rather solutions than statements of the problem.

5. The *Indefinite Standard* takes as the measure of appreciation or depreciation a simple unweighted average of the ratios formed by dividing the price of each commodity at the later period by the price of the same commodity at the earlier period. The average employed may be the Arithmetic Mean used by Soetbeer and many others, or the Geometric Mean used by Jevons, or the Median recommended by the present writer. This standard is recommended by the practice of Jevons [1] and the theory of Cournot.[2] It is discussed in the eighth and ninth sections of the former Memorandum, and the fifth section of the present one.

6. The *Production Standard* takes as the measure of appreciation or depreciation the change in the pecuniary remuneration of a certain set of services, namely, all (or the principal) which are rendered in the course of production, throughout the community, during a year, either at the initial or the final epoch; or some expression intermediate between the two

[1] Most of Jevons' celebrated calculations (*Currency and Finance*, II., III., and IV.), and in particular his calculation of the Probable Error incident to his result (*ibid.*, p. 157), involve this conception.

[2] Cournot has considered our problem in each of the five volumes in which he has treated of, or touched on, Political Economy (*Dictionary of Political Economy*, Art. Cournot). It is sufficient here to refer to the first and the last of those works, the *Recherches* of 1838 and the *Revue Sommaire* of 1876—the Alpha and almost the Omega of economic wisdom. From these it is clear that variation in the "absolute" or "intrinsic" value of money, in Cournot's view, corresponds to the "Indefinite Standard" as defined in Section viii. of the predecessor to this Memorandum. Cournot illustrates the variation due to a change on the part of money, by that change in the position of the earth with respect to the stars, which is due to the motion of the earth. In this analogy the stars are treated as "points" (*Recherches*, Art. 9). No account is taken of their mass. The context shows that Cournot contemplates a simple average of distances between the earth and each star; not a *weighted* average, or the distance between the earth and the *centre of gravity* of the stars. In his later works he expressly declares against, or at least thinks unbefitting highest place, the measure of what he calls the "power of money" (*Review Sommaire*, Sect. 3), that is, in our terms, the Consumption Standard; the analogy of which is the distance of the earth from the *centre of gravity* of the stars, or rather of certain select stars—say those which are nearest to our human sphere. The Currency Standard, of which the analogy is the distance of the earth from the *centre of gravity* of all stars whatever, does not seem to have been entertained by Cournot.

Cournot, alluding to Jevons' treatment of the problem in *Money*, not unjustly takes him to task for not having distinguished "assez nettement" variations in the "intrinsic value of money" [of which the measure is our Indefinite Standard] from variations in the "power of money" [of which the measure is our Consumption Standard] (*Revue Sommaire*, p. 121). Referring to Jevons' proposal to construct a *Tabular Standard of Value*, Cournot expresses his approbation in words which may fittingly conclude the present study :—"Ce sont là des idées qu'il faut laisser mûrir. Quand le moment sera venu de construire effectivement *l'étalon monétaire* les géomètres pourront y trouver une application intéressante de leur *Théorie des Moyennes*, telles qu'ils l'ont déjà construite pour les besoins de l'astronomie et de la physique."

specified. The theoretical basis and practical construction of such a standard are indicated in Ricardo's *Principles of Political Economy* (ch. xx. and elsewhere), in Professor Marshall's evidence before the Gold and Silver Commission (*Parl. Papers* 1888, C. 5,512, Question 9,625), and in the papers contributed by Mr. Giffen to the second volume of the bulletin of the International Statistical Institute. The standard is discussed in the last section of this Memorandum. It is akin to the method suggested in the fifth section of the First Memorandum, and to the standard proposed by Professor Simon Newcomb which was discussed in the first section of this (third) Memorandum.

## (I)

## TESTS OF ACCURATE MEASUREMENT.

[This is the *second* of the Memoranda on Variations in the Value of Money, prepared for the British Association in the latter 'eighties of last century. A separate place is assigned to the second Memorandum, which purports to test the accuracy of the calculations prescribed in the first (and third). The verification is effected by the use of that leading principle of Probabilities known as the Law of Error. Caution is required in applying the test. The beginning of wisdom in this matter is to recognise the analogy between the grouping of heterogeneous price-variations and the dispersion of errors-of-observation. But it is not safe to treat each relative price as an *independent* observation. In the case of prices there is no doubt a good deal of independence in the sense proper to Probabilities. But there is also—as perhaps more than is commonly assumed in the case of physical observations—a good deal of interdependence or correlation among the factors which compose or cause the given observations. The subjoined computations are made in the supposition that each element of an index-number, each percentage representing a comparative price, is "subject to a presumably independent error" (par. 5). So far as this hypothesis does not hold good in the concrete, the inverse square root of $n$, the number of the data, which continually figures in the measure of possible inaccuracy must be taken *cum grano* (cp. *infra*, p. 324).

Attention may be called to the advocacy of the Median (for the computation of certain index-numbers) on the score not only of its peculiar facility, but also (in certain cases) its comparative accuracy. The Weighted Median, Laplace's Method of Situation, is not so familiar an operation but that its exemplification may be useful.

The reader may be assisted in following the computations by having before him an example to which they are often referred, namely, the model index-number proposed by the British Association Committee. It is reprinted here together with the explanations attached to it by Giffen, who took a principal part

in the preparation of the scheme. This index-number and Giffen's explanation formed a part of the Second Report of the Committee which was drawn up by Giffen. The Memorandum by the present writer which is now reprinted originally appeared as an annex to that Second Report. An extract from that Report is appropriately included in these prefatory remarks.

*Extract from the Second Report of the British Association Committee, drawn up by Giffen.*

"The considerations we have to suggest as now most important practically, in preparation for more exact and complete measurements in the future, are the following :—

1. In the absence of retail prices—which it would be most convenient to use in forming a standard of *desiderata*—use must necessarily be made of wholesale prices only. No other prices are obtainable, and those prices must be preferred, in the selection of typical articles, where the records are best.

It appears, however, from the best consideration of the subject, that the differences likely to be made from the true result which would be obtained from a more complete record of prices are not likely to be material. On this head the Committee would refer to a paper by Mr. Edgeworth, which has been prepared for their use, and which is appended. The prices of articles taken without bias from a group are likely to be fairly representative of the average course of prices of that group.

2. While an index-number assigning relative weight to different articles so selected is an important means of arriving at a useful result, it cannot be said, in the present state of the data on the subject, to be an altogether indispensable means. The articles as to which records of prices are obtainable being themselves only a portion of the whole, nearly as good a final result may apparently be arrived at by a selection without bias, according to no better principle than accessibility of record, as by a careful attention to weighting. On this head the Committee may refer to the above paper of Mr. Edgeworth, which seems conclusive on the subject.

3. Practically the Committee would recommend the use of a weighted index-number of some kind, as, on the whole, commanding more confidence. But they feel bound to point out that the scientific evidence is in favour of the kind of index-number used by Professor Jevons—provided there is a large number of articles—as not insufficient for the purpose in hand.

Nothing is more remarkable in the comparisons of the recent index-numbers than the correspondence of the curves of general course of prices indicated. A *weighted* index-number, in one aspect, is almost an unnecessary precaution to secure accuracy, though, on the whole, the Committee recommend it.

4. The Committee have had before them a suggestion for a new index-number, which might be used for some official and private purposes, based on the practical considerations referred to, and making use of the best wholesale prices, while having regard to the ultimate standard of *desiderata*. The nature and object of this index-number are explained in the accompanying memorandum, which has the general approval of the Committee, though they do not consider it necessary here to go into all the details. The object is to provide something for which it would be possible to obtain and publish official prices, and by reference to which contracts could be made, and it is submitted for discussion and future reference.

5. It would be most desirable to supplement any such index-number by a good statistical account from time to time of the aggregate income of the people and the relative numbers and aggregates of incomes of different amounts. In some index-numbers in past times the wage of a day-labourer is inserted as one of the articles. This may have been correct enough for some purposes, and in the circumstances would not prevent the index-number from indicating the general changes in the value of money in the periods compared. But the more useful method would seem to be to distinguish between the human unit in production and the thing produced. Among the most important comparisons for which such figures are used at all are the effectiveness of labour at different times and places, and the command of the labourer or other earner over the amounts produced; and these comparisons can only be made when an independent standard of the production and consumption of the labourer is set up, with which his earnings may be compared. No argument is needed to show that, along with index-numbers as to prices of commodities, there should be an endeavour to ascertain the aggregate earnings of a community and the distribution of the earnings so as to show on the one side the command over commodities which different classes possess—the real as distinguished from the nominal incomes—and on the other side the relative effectiveness of the labour of a community at different times or of one community compared with another.

## Table for the Construction of an Index-number.

*Statement showing the estimated amount of the expenditure on the undermentioned articles in the United Kingdom and the proportion of the amount in each case to the total expenditure on all such articles, with suggestions for an index-number based approximately on the proportions stated, but with modifications so as to substitute percentages in round figures; showing also the description of the specific wholesale article, the price of which it is proposed to use in the calculation of the index-number; giving also the price-list or other source from which quotations are to be obtained.*

| 1 | 2 | 3 | 4 | 5 | 6 | 7 |
|---|---|---|---|---|---|---|
| Heads of articles. | Articles consumed or used up. | Estimated expenditure per ann. on each article. | Percentage of each amount in column 3 to total. | Relative importance proposed for each article in index-number reduced to percentages. | Description of the specific article of which the price is to be quoted as typical. | Price-list or other source for price quotations. |
| Breadstuffs. | | £ 000,000 | | | | |
| | Wheat . | 60 | 6·5 | 5 | English wheat . . | Gazette average |
| | Barley [1] . | 30 | 3·25 | 5 } 20 | ,, barley . . | ,, ,, |
| | Oats . . | 50 | 5·4 | 5 | ,, oats . . . | ,, ,, |
| | Potatoes, rice, etc. | 50 | 5·4 | 5 | ,, potatoes . | Average import price |
| Meat and dairy food. | Meat . . | 100 | 11 | 10 | Mean of live meat per stone of 8 lbs. Smithfield | Weekly market quotations |
| | Fish . . | 20 | 2·2 | 2½ } 20 | Average per cwt. landed | Official returns (Board of Trade) |
| | Cheese Butter Milk . | 60 | 6·5 | 7½ | Cheese . . . . Butter . . . . | Average import price ,, ,, ,, |
| Mass luxuries. | Sugar . | 30 | 3·3 | 2½ | Refined sugar imported | Average import price |
| | Tea . . | 20 | 2·2 | 2½ | Tea imported . . | ,, ,, ,, |
| | Beer . . | 100 | 11 | 9 } 20 | Beer exported . . | Average export price |
| | Spirits . | 40 | 4·3 | 2½ | Spirits imported . | Average import price |
| | Wine . | 10 | 1 | 1 | Wine imported . . | ,, ,, ,, |
| | Tobacco . | 10 | 1 | 2½ | Tobacco imported . | ,, ,, ,, |
| Clothing. | Cotton . | 20 | 2·2 | 2½ | Cotton imported . | Average import price |
| | Wool . | 30 | 3·3 | 2½ } 10 | Wool imported . . | ,, ,, ,, |
| | Silk . . | 20 | 2·2 | 2½ | Raw silk imported . | ,, ,, ,, |
| | Leather . | 10 | 1·1 | 2½ | Hides imported . | ,, ,, ,, |
| Metals and minerals. | Coal . . | 100 | 11 | 10 | Coal exported . . | Average export price |
| | Iron . . | 50 | 5·4 | 5 } 20 | Scotch pig-iron . . | Market price |
| | Copper . | 25 | 2·7 | 2½ | Copper ore imported | Average import price |
| | Lead, zinc, tin, etc. | 25 | 2·7 | 2½ | Lead ore imported . | ,, ,, ,, |
| Miscellaneous. | Timber . | 30 | 3·3 | 3 | Timber imported . | Average import price |
| | Petroleum | 5 | ·6 | 1 | Petroleum imported | ,, ,, ,, |
| | Indigo . | 5 | ·6 | 1 | Indigo imported . | ,, ,, ,, |
| | Flax and linseed | 10 | 1·1 | 3 } 10 | Flax imported . . | ,, ,, ,, |
| | Palm oil . | 5 | ·6 | 1 | Palm oil imported . | ,, ,, ,, |
| | Caoutchouc | 5 | ·6 | 1 | Caoutchouc imported | ,, ,, ,, |
| | Totals . | 920 | 100 | 100 | | |

[1] There is a large consumption of barley, exclusive of its use in the manufacture of beer.

"In this table the first column indicates six leading genera which comprehend the twenty-seven classes of articles specified in the second column. These articles are either finished products (things ready for consumption, like cheese and milk) or *represent* such things by entering into their production, as coal (used in manufacturing) and timber, for instance, go to the production of houses and furniture.

"The third column gives in round numbers (000,000's being omitted) the average national expenditure on each class of article at present and for the last few years, and presumably also for the immediate future the *proportions* at least, if not the absolute amounts, of expenditure (such proportions, as shown in Mr. Giffen's reports on the variation in the prices of exports and imports, remaining pretty constant during a period of years). In the estimated amount of consumption allowance is made for the addition to the value made before the articles are in the form in which they are finally consumed.

"In column 4 these amounts (or proportions) are reduced to percentages (of the total amount expended on such articles).

"In column 5 the relative importance proposed to be assigned to each article in the index-number is stated, mainly on the basis of the percentages in column 4, but with modifications so as to substitute even figures for the convenience of handling.

"In column 6 the specific articles are described, of which it is proposed to obtain the prices as typical of the group really included on the corresponding line in column 2. Wheat, for instance, consists of many different kinds and qualities; the one quality and kind it is proposed to quote as typical of the whole is English wheat as returned officially to the Comptroller of the corn returns, which itself no doubt comprises many qualities. Of iron, again, there are innumerable qualities and kinds; it is proposed to take Scotch pig-iron, in which there are large dealings, as typical of the whole. The same with other articles. In most cases large groups are dealt with because the article selected is the average imported or exported, which includes many qualities, but it should be distinctly understood that in any case the most that can be done is to select specific articles which are typical of large groups.

"In column 7 the source from which the quotation of the specific articles mentioned in column 6 is to be obtained is stated.

"The above is of course only a rough suggestion for an index-number. Even if the method is generally approved of, many questions might be discussed as to the amounts of the annual

consumption of each group of articles specified in column 2, as to the relative importance to be assigned practically in column 5, and as to the selection of the article in column 6 which is to be treated as typical of the group. It would be possible to introduce two or more quotations instead of one for a particular group if thought desirable, but this would be troublesome in working. For practical purposes there must not be too many articles. Mr. Edgeworth's mathematical deductions as to the consequences of taking the price of an article selected at random from a group, instead of the general average course of prices for the group, appear to justify the expediency of this procedure.

"Were such a general index-number introduced, and prices calculated upon it backwards and forwards, it would be easy to rearrange it for any special purpose, such as to give more or less weight to one or more groups according as they are assumed to enter into the consumption of a particular class of persons whose position at different times as affected by the course of prices is to be specially investigated. The index-number could also be compared with other index-numbers upon some other objective basis, such as the relative importance of each article in the import and export trade of a country; and index-numbers for one country and place could be compared with those for other countries or places. The index-number now suggested is only put forward as a convenient one, illustrating the variations in prices in England according to what is called the standard of *desiderata*, and which could be made use of—not neglecting others—in many investigations.

"It would also be an index-number on which, if people were so inclined, they could make contracts in a way analogous to the contracts for the commutation of tithe; in which the tithe is made to vary according to the prices of corn. To make the index-number useful for this purpose an Act would have to be passed prescribing the way in which the prices are to be obtained and published, and defining and giving a form for the contracts which might be made for payments, to vary according to the variation in the aggregate index-number. This would be a practical Tabular Standard such as Joseph Lowe, Jevons, and lately Professor Marshall, have suggested.

"All such index-numbers are liable to the observation that innumerable articles are, and must be, in the nature of things, wholly excluded. The variety of small articles is almost infinite. The assumption may also be made, I think, that on balance the permanent tendency is for such articles on the average,

through the progress of invention, to increase in aggregate importance in proportion to the other articles which can be got into an index-number and, at the same time, individually to fall relatively in price. In investigations general facts of this kind would, of course, have to be borne in mind as qualifying deductions based upon the precise figures which the index-numbers may give. People making contracts based on index-numbers would also require to study what the effect would be likely to be on the result they wish to arrive at."—*End of Extract.*]

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ANALYSIS OF CONTENTS.



The usefulness of our result will be enhanced by an estimate of its accuracy. It would be desirable, if possible, to ascertain a numerical limit which the *error* [1] incurred by our calculation cannot possibly, or at least with any reasonable probability, exceed. But it is doubtful whether such a limit admits of being fixed with precision. The erroneousness of the conclusion could only be ascertained by inference from the inaccuracy of the premises. But it is difficult to appreciate with mathematical precision the error to which our data are liable. We may, however, argue that, if the erroneousness of the premises is approximately of a certain amount, if the error of the data is of a certain *order*, then the error of the conclusion will be of a certain other order.

[1] The use of the term "error" to denote a deviation from an unknown ideal is somewhat infelicitous. But the advantage which the term has in being familiar to the student of Probabilities may, it is hoped, preponderate over the disadvantage that it suggests to the general reader a more gross, blameworthy, and avoidable mistake than is contemplated here.

The subject of our investigation being thus defined, we may show that the erroneousness of the result is *less* than that of the data. There are two lines of proof converging to the truth of this theory. *First*, we may reason *a priori* by the Calculus of Probabilities that the index-number is subject to a smaller percentage of error than the weights and relative prices (given or referred to in columns 5 and 6 of the table). *Secondly*—this deduction may be verified by actual trial. We may assign a certain set of weights and relative prices as correct, and construct several sets of variants diverging from the "correct" figures in haphazard fashion. Then, operating with each set of variant data, we may calculate several variant index-numbers. These, it will be found, diverge less—that is by a smaller percentage—from the correct index-number than any set of variant data from the corresponding correct datum.

The second part of the evidence cannot be fully appreciated without the prior reasoning. By itself it conveys only a moiety of the truth. Those who are content with that fraction of knowledge are advised to skip the reasoning of the immediately following paragraphs, and to pass on to the more easily read lessons of experience (at p. 312 below).

The index-number which is the result of our calculations is subject to a less error than the data which enter into it, for two reasons. *First:* The numerator and denominator of the fraction which constitutes the index-number form each an aggregate of elements or parts, whereof each element is subject to a presumably independent error. Now, by a well-known principle of the Calculus of Probabilities, the percentage error of such an aggregate is less than the percentage error incident to each element (or at least to an element of average erroneousness). This principle applies to the errors both of the *weights* and the *observations* (relative prices).* The next consideration applies only to the former class of data. An error in any *weight* affects both the numerator and denominator in the same direction, whether of excess or defect, and thus is to a certain extent self-corrected.

This reasoning may be exhibited more fully by the aid of symbols. Let us put the series $p_1$, $p_2$, etc. . . . $p_n$ for the *real* relative prices. These relative prices may be conceived as percentages obtained after the manner of Mr. Palgrave (see Table 26 of Memorandum in Appendix to "Third Report of the Commission on Depression of Trade") by multiplying the ratio

* The term "price-variation" was employed for what is now substituted "relative price."

$\frac{\text{New price}}{\text{Old price}}$ by 100. Let us denote the *apparent* relative prices, the erroneous observations, as $p_1(1 + e_1)$, $p_2(1 + e_2) \ldots p_n(1 + e_n)$, where $e_1$, $e_2 \ldots e_n$ are each positive or negative errors, usually proper fractions. Similarly let $w_1$, $w_2$, etc., be the *real* weights; and $w_1(1 + \epsilon_1)$, $w_2(1 + \epsilon_2) +$ etc., be the apparent, or erroneous, weights.

The index-number obtained from such data is

$$\frac{w_1(1 + \epsilon_1) \times p_1(1 + e_1) + w_2(1 + \epsilon_2) \times p_2(1 + e_2) + \text{etc.}}{w_1(1 + \epsilon_1) + w_2(1 + \epsilon_2) + \text{etc.}}.$$

Alike in the numerator and denominator of this expression we may segregate the *correct* and the *erroneous* portion; and reason by the first of the principles above mentioned that the incorrect portion is of a smaller order than the sum of the correct terms (the number of observations being sufficiently great). Accordingly it will be allowable to expand by Taylor's Theorem and neglect higher terms. We shall thus obtain a simple expression for the error of the resultant index-number in terms of the errors to which each class of the data is liable.

This investigation may be broken up into three steps: we may consider successively three cases in an order of increasing complexity. First (1) we shall suppose that the weights only are liable to error. Then (2) we shall introduce the circumstance that the observations, the relative prices, are themselves incorrect. Lastly (3) we shall take account of the fact that certain categories of articles may be altogether unrepresented.

(1) Under the first head we shall first consider the simple case when the weights are really equal, though apparently somewhat unequal. In this preliminary case the symbolic expression above written becomes simplified by the disappearance both of the $e$'s and the $w$'s. Expanding and segregating the heterogeneous elements in the manner indicated, we may write our result thus:—

$$\frac{p_1 + p_2 + \text{etc.}}{n}\left\{1 + \frac{p_1\epsilon_1 + p_2\epsilon_2 + \text{etc.}}{p_1 + p_2 + \text{etc.}} - \frac{\epsilon_1 + \epsilon_2 + \text{etc.}}{n}\right\},$$

where the term outside the brackets is the *correct* index-number, and the difference of the second and third terms within the brackets is the error of the index-number: the relative error, as it may be called, or (if multiplied by 100) the percentage error, in symbols $\frac{\Delta \mathrm{I}}{\mathrm{I}}$, if I is the correct index-number. The result obtained may be written

$$\frac{\Delta \mathrm{I}}{\mathrm{I}} = \frac{\mathrm{S}p}{n}\left\{1 + \epsilon_1\left(\frac{p_1}{\mathrm{S}p} - \frac{1}{n}\right) + \epsilon_2\left(\frac{p_2}{\mathrm{S}p} - \frac{1}{n}\right) + \text{etc.}\right\}.$$

In this expression call the factors of $\epsilon_1$, $\epsilon_2$, etc., respectively $\frac{1}{n}E_1$, $\frac{1}{n}E_2$, etc. Then $\frac{\Delta I}{I}$, the error whose magnitude we have to estimate, is $\frac{1}{n}(E_1\epsilon_1 + E_2\epsilon_2 + \text{etc.})$. To determine the probable and improbable limits of this quantity we require to know the magnitude, or at least the average extent, both of the E's and the $\epsilon$'s. The former datum depends upon the dispersion of the observations (the comparative prices) about their mean. For any E, *e.g.*—

$$E_r = n\left(\frac{p_r}{Sp} - \frac{1}{n}\right) = n\frac{\left(p_r - \frac{Sp}{n}\right)}{Sp} = \frac{\left(p_r - \frac{Sp}{n}\right)}{\frac{Sp}{n}}$$

= the deviation or *error* incurred by the individual relative price as compared with the average of a whole set; *relative* to (divided by) the average. Such a deviation might be symbolised as $\frac{\Delta p}{p}$, if we put $p$ for the average relative price.

We may now proceed in two ways: ($\alpha$) we may either suppose the deviations $E_1$, $E_2$, ascertained for the particular year or epoch to which the calculation in hand may refer; ($\beta$) or we may seek a measure for general use, and available without the trouble of examining the dispersion of the relative prices for a particular year. In either case we are to regard the $\epsilon$'s as errors grouped in random fashion about a mean, which is zero. The coefficient which measures the dispersion of these errors, the *modulus* for the $\epsilon$-fluctuation, must be supposed knowable. Call it $\kappa$.

($\alpha$) On the former understanding, we are to regard $E_1$, $E_2$, etc., as known factors. Accordingly by a well-known theorem the *modulus*, which measures the extent of the error

$$\frac{1}{n}(E_1\epsilon_1 + E_2\epsilon_2 + \text{etc.}) = \frac{1}{n}\sqrt{E_1^2 + E_2^2 + \text{etc.}} \times \kappa,$$

$$= \frac{1}{\sqrt{n}}\sqrt{\frac{E_1^2 + E_2^2 + \text{etc.}}{n}} \times \kappa,$$

($\beta$) Otherwise we are to regard $E_1$, $E_2$, as samples, so to speak, taken from an indefinite number—a complete series (in Dr. Venn's phrase) of E's. We must suppose the coefficient of fluctuation, or modulus, for this series to be given by prior experience. Let it be C. Then we may put as the most probable value for the measure or *modulus* of $\frac{\Delta I}{I}$, the error under consideration,

$$\frac{1}{\sqrt{n}} \times \frac{C}{\sqrt{2}} \times \kappa.$$

But this *most probable* measure may conceivably not be the *best* measure.* We must take into account that the real measure *may* be larger, and accordingly that, by adopting the measure described as "most probable," we may be underrating the probability of each extent of deviation (from zero) to which the quantity $\frac{1}{n}[E_1\epsilon_1 + E_2\epsilon_2$, etc.] is liable. However, the error thus introduced is only of the order $\frac{1}{\sqrt{n}}$, that is, the $\frac{1}{\sqrt{n}}$th part of the magnitude to be evaluated. Now that degree of error has been already incurred by the neglect of the higher terms in the expansion of $\frac{\Delta I}{I}$. Accordingly it would be nugatory to apply correctives to the error now under consideration.

We have now to introduce the circumstance that the weights, both real and apparent, differ from unity. It may be shown that in the new expression for $\frac{\Delta I}{I}$ the coefficient of any weight-error $\epsilon_r$ is $\frac{w_r p_r}{Swp} - \frac{w_r}{Sw}$; which may be put in the form $\frac{w_r}{Sw}E'_r$, where $E'_r$ is now the proportional deviation of $p_r$ from the *weighted* mean of the $p$'s, viz. $\frac{Swp}{Sw}$. Accordingly the modulus of $\frac{\Delta I}{I}$ becomes

$$\frac{\sqrt{w_1{}^2E'^2 + w_2{}^2E'^2 + \text{etc.}}}{Sw}\kappa.$$

In evaluating the coefficient of $\kappa$ there are, as before, two courses. Either ($\alpha$) we operate upon the known values of $E'_1$, $E'_2$, etc., for the particular year or epoch with which we are concerned. Or ($\beta$) we may make a general estimate based upon several years' experience, and roughly applicable to the unexamined data of any year.

($\alpha$) In the former case there is nothing more to be said, except that it will be legitimate in the evaluation of the modulus to put for $w_1$, $w_2$, etc., their *apparent* values; which may be written $w_1 + \Delta w_1$, $w_2 + \Delta w_2$, etc. For the error thus introduced into the modulus is of a negligible order.

($\beta$) The general expression in terms of the E-fluctuation is found by considering that the most probable value of the quantity under the radical sign in the last written expression is $\sqrt{(w_1{}^2 + w_2{}^2 + \text{etc.})}\frac{C^2}{2}$, where $\frac{C^2}{2}$ is the mean square of error

* Or is it sufficient to say that, C and $\kappa$ being uncorrelated, the expectation of their product = the product of their expectations? (cf. *Ency. Brit.*, Art. "Probability," § 15).

measured, not, as before, from the simple (arithmetical) mean (of many batches of $p$'s), but from the weighted mean $\frac{Swp}{Sw}$; a difference which may be shown as follows to be of an order which may for our purpose be neglected.

The deviation of any $p$ from the Weighted Mean—the relative or proportionate deviation—E′

$$= \frac{\frac{Spw}{sw} - p_r}{\frac{Spw}{sw}}.$$

This ratio may be thus expressed in terms of $E_r$, the deviation of $p_r$ from the *Simple Arithmetic* Mean. Put $v$ for the difference between the weighted and simple means. Then we have

$$E'_r = \frac{\left(\frac{Sp}{n} - p_r\right) - v}{\frac{Sp}{n} - v} = \frac{E_r - \frac{v}{p}}{1 - \frac{v}{p}},$$

if we put $p$ for the Arithmetic Mean of the $p$'s.

$$\text{Now } v = \frac{Sp}{n} - \frac{Spw}{Sw} = \frac{p_1 + p_2 + \text{etc.}}{n} - \frac{p_1 w_1 + p_2 w_2 + \text{etc.}}{w_1 + w_2 + \text{etc.}}.$$

Substitute for $p_r$ its value $p(1 + E_r)$ (where $p$ is the Arithmetic Mean of the $p$'s); and we have

$$v = p\left[\frac{E_1 + E_2 + \text{etc.}}{n} - \frac{w_1 E_1 + w_2 E_2 + \text{etc.}}{w_1 + w_2 + \text{etc.}}\right]$$

$$= \frac{1}{n} p E_1 \frac{\frac{Sw}{n} - w_1}{\frac{Sw}{n}} + \frac{1}{n} p E_2 \frac{\frac{Sw}{n} - w_2}{\frac{Sw}{n}} + \text{etc.}$$

Put for the relative deviation of any $w$ from the Arithmetic Mean of all the $w$'s (the coefficient of $\frac{1}{n}pE_r$ in the last written expression) $\eta_r$. Then we have

$$v = \frac{1}{n} p [E_1 \eta_1 + E_2 \eta_2 + \text{etc.}].$$

The expression in brackets hovers about the value zero according to a law of error whose modulus is $\frac{\sqrt{n} C\chi}{\sqrt{2}}$; where C, as before, is the modulus of the E's, and $\frac{\chi^2}{2}$ is the mean square of the $\eta$'s.

Hence $\frac{v}{p}$ is of an order $\sqrt{n}$ times smaller than $C\chi$.

Now from the equation connecting E′ and E it appears that the sum of squares $E'^2_1 + E'^2_2 +$ etc. which occurs in the complete expression for the modulus of $\frac{\Delta I}{I}$ may be written

$$\frac{SE_r^2 + n\left(\frac{v}{p}\right)^2}{\left(1 - \frac{v}{p}\right)^2};$$

whence, as $SE_r^2 = n\frac{C^2}{2}$, it appears that the influence of $\frac{v}{p}$ may be neglected, $n$ being supposed large.

We may therefore write

$$\text{Modulus of } \frac{\Delta I}{I} = \frac{\sqrt{Sw_r^2}}{Sw}\,\frac{C}{\sqrt{2}}\kappa;$$

or, employing the notation which we had lately occasion to introduce:

$$\text{Modulus of } \frac{\Delta I}{I} = \frac{1}{\sqrt{n}} \times \sqrt{1 \times \frac{\chi^2}{2}} \times \frac{C}{\sqrt{2}} \times \kappa.$$

This formula may be employed to utilise present as well as past experience. If we treat $\frac{\chi^2}{2}$ and $\frac{C^2}{2}$ as respectively the mean square of deviation obtained from the set of weights and price-returns entering into the index-number which we are computing, we shall thus have an approximate formula more convenient than the complete expression for the Modulus.

(2) We have now to introduce the circumstance that each $p$ is liable to an error $pe$. Each element of error of the form $\Pi\epsilon_r$ is now aggravated by an element of the form $Pe_r$. Accordingly the modulus of the total error will be $\sqrt{\Pi^2\kappa^2 + P^2c^2}$, where $\kappa$ and $c$ are the moduli for the independent partial errors of the weights and the prices respectively, $\Pi$ is the coefficient of $\kappa$ in the expression for the modulus of $\frac{\Delta I}{I}$ in case (1) and $P^2$ is equal to $\frac{Sw_r^2p_r^2}{(Swp)^2}$.

There may now be required, as before, a general formula applicable without any examination of the prices and weights on a particular occasion; or without other data than the coefficients expressing the dispersion of the prices and weights respectively. With this view, employing the notation already explained, and rejecting terms which may be shown to be of an inferior order, we may put for

$$\frac{Sw_r^2p_r^2}{(Swp)^2} \text{ the expression } \frac{1}{n}\left(1 + \frac{\chi^2}{2}\right)\left(1 + \frac{C^2}{2}\right).$$

Hence for the modulus of $\frac{\Delta \mathrm{I}}{\mathrm{I}}$ in the general case we have

$$\frac{1}{\sqrt{n}} \times \sqrt{1 + \frac{\chi^2}{2}} \times \sqrt{\frac{\mathrm{C}^2}{2}\kappa^2 + \left(1 + \frac{\mathrm{C}^2}{2}\right)c^2}.$$

(3) So far we have been estimating the errors due to the weights and prices of the articles which enter into our index-number not being accurate. We have now to take into account that not only are all those articles misrepresented, but also that certain other articles may be wholly unrepresented. For it is unlikely that all the classes of products which ought by rights to enter into an index-number can, even constructively, put in an appearance.

We have now to superinduce the error due to such omission upon the errors already estimated. To effect this we proceed in the same way as when compounding the errors proper to our first and second headings. That is, we shall separately evaluate for the third species of error its modulus squared, or *fluctuation*, as the present writer has proposed to term this important coefficient. Then we shall add the third fluctuation to the sum of the two preceding: that is, to the square of the formula given at the end of the second heading.

To find the fluctuation proper to the third heading, let us begin with the simple case in which the weights are all equal. As before, let $\mathrm{S}p$ represent the sum of the observed (comparative) prices; let $n$ be their number; and for $\frac{\mathrm{S}p}{n}$ put simple $p$. Let $\mathrm{S}'p$ be the sum, and $n'$ the number, of the *unobserved* prices. Then the error incurred by putting $p$ for the Mean of all the prices, the relative error $\frac{\Delta \mathrm{I}}{\mathrm{I}}$, is

$$\left\{\frac{\mathrm{S}p + \mathrm{S}'p}{n + n'} - \frac{\mathrm{S}p}{n}\right\} \div \frac{\mathrm{S}p + \mathrm{S}'p}{n + n'}.$$

The most probable value of this expression is zero; while its *fluctuation* is found to be, in terms and by methods already explained,*

$$\frac{1}{n^2} \times \frac{2nn'}{(n + n')} \times \mathrm{C}^2.$$

Now superadd the circumstance that the weights are various, dispersed about their mean according to the modulus $\chi$; and connect the resulting expression with the square of the formula given at the end of heading (2).

The formula will require modification, if there is reason to

* That is treating the supposed complete set of observations obtained at one time as a *specimen* of a series obtained at other times (*cp.* above, p. 215). Otherwise we may regard $\mathrm{S}p + \mathrm{S}p'$ as the "universe," of which $\mathrm{S}p$ constitutes a *sample*

believe that the omitted articles have not the same average weight as those which are included; for instance, if, as is likely, the omissions are many in number, but inconsiderable in weight.

It will be noticed that in passing from (the dispersion of) the observed prices and weights to what has not been observed there is an inductive hazard greater than is involved by solutions of cases (1) and (2) in their more exact form, and while we suppose (as in the examples which will be adduced below) that the errors of weight and price emanate from regular and stable sources, so as to admit of safe prediction.

As in case (2), we may suppose the coefficients $\chi$ and C based either on prior experience or on the data appertaining to the particular calculation which is in hand.

It will be observed that these coefficients do not contribute equally to the resultant error represented by our formula. C, expressing the dispersion of the prices, is more efficacious than $\chi$, appertaining to the weights. Similarly $c$, the measure of the error incident to the prices, affects the error of the index-number more than $\kappa$, the corresponding modulus of the weights.

It is proposed now to illustrate the formulæ which have been given by working a few examples. In these examples the statistical materials, the prices and weights, are taken out of Mr. Palgrave's Memorandum, from Tables 26 and 27 respectively. The conjectural arbitrary assumptions which will be made are that any price, and likewise any weight, is as likely as not to be out, in excess or defect, of the true figure by 10 per cent., but very unlikely to be out by 40 per cent., or, more exactly, that the apparent values fluctuate about the real one in conformity with a modulus which is 21 per cent.

Of the immense variety of cases which might be constructed by combining in different ways the attributes which define the preceding paragraphs, it will be sufficient here to discuss the most important case (2) of both weights and prices subject to error—divided into two species, according as ($\alpha$) we utilise all the data special to the calculation in hand, or ($\beta$) content ourselves with the more summary estimate.

Let us apply these tests to Mr. Palgrave's computation of a weighted mean for the year 1885 (Memorandum in Appendix to *Third Report on the Depression of Trade*, 1886, C. 4797). First, according to method ($\alpha$), the expression for the (proportionate) error due to a particular element of the index-number, the weight and price of a particular commodity, is

$$\epsilon_r \frac{w_r}{Swp}\left[p_r - \frac{Swp}{Sw}\right] + e_r \frac{w_r}{Sw} p_r.$$

Whence, as the Modulus of the error to which the computed index-number is liable, we have—putting $p'$ for the *weighted mean* of the price-returns, and remembering that $c$ and $\kappa$ are the Moduli of the errors $e$ and $\epsilon$ respectively—

$$\frac{1}{Swp}\sqrt{Sw_r^2(p' - p_r)^2\kappa^2 + Sw_r^2 p_r^2 c^2}.$$

The $w$'s are given in Mr. Palgrave's column headed "Relative Importance" (Table 27, year 1885, p. 35). The $p$'s are to be extracted from his Table 26. The weighted mean $p'$ is, according to him, 76. And $Swp$ is the sum of his column (for the year 1885), headed "Comparative," etc., *multiplied by* 100; that is 166,900. The rest of the expression above written is evaluated in the following table; of which the materials are taken from the sources named. The third column is formed by subtracting from each of the entries for 1885 in Mr. Palgrave's Table 26—*e.g.*, 38 the price of cotton (comparative with 1865–9)—the weighted mean 76. The last three columns in Mr. Palgrave's Table 26, relating to *Cotton Wool, Cotton Yarn*, and *Cotton Cloth*, are omitted, as they do not figure in his Table 27, and, it may be added, cannot be supposed *independent* of the price of cotton. The last column in our table is formed by squaring each entry in Mr. Palgrave's column headed "Comparative," etc. (Table 27, year 1885), and omitting the last digit :—

| No. of Article. | Name of Article. | $w$. | $w^2$. | $(p_r - p')$. | $(p' - p_r)^2$. | $w^2(p' - p_r)^2$. | $w_r^2 p^2$. |
|---|---|---|---|---|---|---|---|
| | | | 00's omitted | | | 00,000's omitted | 00,000's omitted |
| 1 | Cotton . . . | 263 | 691 | −38 | 1,444 | 998 | 1,000 |
| 2 | Silk . . . . | 12 | 1 | −23 | 529 | 0 | 4 |
| 3 | Flax, etc. . . | 49 | 24 | −15 | 225 | 0 | 90 |
| 4 | Wool . . . . | 142 | 202 | − 7 | 49 | 10 | 980 |
| 5 | Meat . . . . | 524 | 2,745 | +26 | 676 | 1,855 | 28,622 |
| 6 | Iron . . . . | 150 | 225 | + 6 | 36 | 8 | 1,510 |
| 7 | Copper . . . | 39 | 15 | −27 | 729 | 11 | 53 |
| 8 | Lead . . . . | 13 | 2 | −19 | 341 | 1 | 5 |
| 9 | Tin . . . . | 15 | 23 | + 2 | 4 | 0 | 14 |
| 10 | Timber . . . | 164 | 269 | +31 | 961 | 258 | 3,099 |
| 11 | Tallow . . . | 28 | 8 | + 8 | 64 | 0 | 58 |
| 12 | Leather . . . | 80 | 64 | +34 | 1,156 | 73 | 774 |
| 13 | Indigo . . . | 5 | 0 | +35 | 1,225 | 0 | 4 |
| 14 | Oils . . . . | 49 | 24 | − 7 | 49 | 1 | 116 |
| 15 | Coffee . . . | 8 | 1 | −14 | 196 | 0 | 3 |
| 16 | Sugar. . . . | 149 | 223 | −23 | 576 | 128 | 624 |
| 17 | Tea . . . . | 71 | 50 | − 7 | 49 | 2 | 240 |
| 18 | Tobacco . . . | 29 | 8 | +27 | 729 | 6 | 90 |
| 19 | Wheat . . . | 410 | 1,681 | −16 | 256 | 430 | 5,856 |
| Sums | | 2,200 | 6,256 | | | 3,781 | 43,142 |

According to the hypotheses above made let us put $c$ and $\kappa$ each $= \cdot 21$. Then for the sought Modulus we have

$$\cdot 21 \frac{\sqrt{378{,}100{,}000 + 4{,}314{,}200{,}000}}{166{,}900} = \cdot 21 \times \cdot 41 \text{ (nearly).}$$

Thus the error incident to each of the data has been reduced by more than a half in the result. It may be observed that the prices contribute much more largely than the weights to the total error. If we reduce the error incident to each price-return by a half, making its modulus ·1, instead of ·21, the total error of the result will be reduced by nearly a half—from modulus ·086 to modulus ·046. If we suppose the price-returns to be quite correct, then the error of the result due to the weights alone would be nearly half as small again, namely, of modulus ·025. This is agreeable to what was said above, that an error of the prices affecting only the numerator of the index-number is not, as in the case of the weights, compensated by an error affecting the denominator in the same degree.

Let us see now ($\beta$) how we should have fared if we had based our estimate on the grouping of the weights and prices in prior experience, such as is afforded by the table of prices cited from the *Economist*.

The dispersion of the price-returns, the coefficient C in the general formula, is thus to be found—in the case of the year 1884 for example. The arithmetic mean of the first nineteen entries in Table 26 for 1884 is 81 nearly. The "differences" and squares of differences are computed in the accompanying table. The

| Name of Article. | Price. | Differences. | | | Squares of Differences. |
|---|---|---|---|---|---|
| | | — | | + | |
| Coffee . . . | 70 | 11 | | | 121 |
| Sugar . . . | 77 | 4 | | | 16 |
| Tea . . . | 81 | | 0 | | — |
| Tobacco . . | 90 | | | 9 | 81 |
| Wheat . . . | 73 | 8 | | | 64 |
| Meat . . . | 103 | | | 22 | 484 |
| Cotton . . . | 37 | 44 | | | 1,936 |
| Silk . . . | 66 | 15 | | | 225 |
| Flax, etc. . . | 59 | 22 | | | 484 |
| Wool . . . | 73 | 8 | | | 64 |
| Indigo . . . | 107 | | | 26 | 676 |
| Oils . . . | 81 | | 0 | | — |
| Timber . . . | 105 | | | 24 | 576 |
| Tallow . . . | 109 | | | 28 | 784 |
| Leather . . | 106 | | | 25 | 625 |
| Copper . . . | 70 | 11 | | | 121 |
| Iron . . . | 76 | 5 | | | 25 |
| Lead . . . | 61 | 20 | | | 400 |
| Tin . . . | 90 | | | 9 | 81 |
| Sums . . . | ... | 148 | | 143 | 6,763 |

mean square of difference, 353, divided by the square of the mean 6561 forms an approximate, a *prima facie* value for $\frac{C^2}{2}$, namely, ·04.

$$\text{Mean square of deviation} = \frac{6765}{19} = 353.$$

For the year 1880, taken similarly as a random specimen, the mean (of the nineteen prices) is found to be 93·5, and the mean square of differences 434. Accordingly the value for $\frac{C^2}{2}$ is ·05. Proceeding similarly for 1873, another year taken at random, we find for $\frac{C^2}{2}$ again ·05. As the mean of the three values we may put ·05.

To find the dispersion of the $w$'s we proceed similarly. The arithmetical mean is for every year 2200 ÷ 19, or 116 nearly. The "differences" are to be formed by subtracting this figure from each of the entries in the column headed *Relative Importance*, in Mr. Palgrave's Table 27. The sum of the squares of the differences is to be divided by 19 for the absolute mean square of difference as it may be called. This result, divided by $116^2$, gives the mean square of deviation relatively to the mean weight. The values thus extricated for the years 1873, 1880, and 1884 respectively are, in round numbers, 354,000, 351,000, 357,000: each divided by 255,664 ($= 19 \times 116^2$); whereof the mean value is 1·38.

Substituting in the general or summary formula given under head (2) for the modulus of $\frac{\Delta I}{I}$ the values for $C^2$ and $\chi^2$ just ascertained, and for $c$ and $\kappa$ the assumed value ·21, we have

$$\frac{1}{\sqrt{19}} \times \sqrt{2{\cdot}38} \times \sqrt{{\cdot}05 \times {\cdot}044 + 1{\cdot}05 \times {\cdot}044} = \frac{1}{4{\cdot}36} \times 1{\cdot}54 \times {\cdot}22 \text{ (nearly)} = {\cdot}077;$$

whereas the answer found by the more exact method was ·086. This consilience seems greater than might have been expected, considering the small number of the elements entering into the computation, only nineteen; and the scantiness of the induction by which we determine the coefficients C and $\chi$.

If we employ the summary formula as a short method of utilising the data special to the index-number of 1885, we shall find that $\frac{C^2}{2}$ as based upon the fluctuation of prices for this year

is ·08; and $\frac{\chi^2}{2}$ the mean square of deviation for the $w$'s is still 1·38. Hence, as the approximate expression for the modulus, we have

$$\frac{1}{4{\cdot}36} \times 1{\cdot}54 \times {\cdot}21\sqrt{1{\cdot}16} = {\cdot}08.$$

Thus we reach much the same result by the shorter as by the more tedious route.

We shall presently—in the portion of this paper addressed to the general reader—try an experiment calculated to verify our deductive reasoning—so far as a theorem in the Calculus of Probabilities can be verified by a single experiment. We shall affect each of the elements in Mr. Palgrave's index-number for 1885, each weight and price, with a figure taken at random from a series of figures hovering about unity in conformity with a modulus equal to ·21. Such a series the writer happens to have ready to hand: consisting of sums of twenty digits taken at random from mathematical tables, where the mean value is 90 and the absolute modulus 19. The relative modulus, therefore, the modulus for the series when we divide each aggregate by 90, is ·21. Accordingly it will be sufficient to multiply each weight both in the numerator and the denominator with one of the sums (of twenty digits) taken at random, and similarly affect each price entering into the numerator, while the denominator is multiplied by 90.

To resume now, in popular language, this somewhat technical inquiry. The subject under investigation is the error to which our computation of index-numbers is liable—the error relative to, or per cent. of, the true value which we seek. We want to know, for instance, whether it is as likely as not that our calculation exceeds (or falls short of) the correct result by 10 per cent. of that result; whether it is very improbable that the excess (or defect) should be as great as 25 per cent.

The error thus conceived is found to depend in a definite manner upon *six* distinct circumstances. The erroneousness of the result is greater, the greater the inaccuracy of the data: the weights and the (comparative) prices. The erroneousness of the result is also greater, the greater the inequality of the weights, and the greater the inequality of the price-returns. Lastly, the result is more accurate, the greater the number of the data and the smaller the number of omitted articles.

These circumstances are not all equally operative. Other things being the same, the inaccuracy of the price-returns affects

the result more than inaccuracy of the weights; and the inequality of the price-returns more than the inequality of the weights.

The only proof of the theory which can be offered to the unmathematical reader is to verify it by actual trial. We may assume a certain set of data as perfectly correct: then affect each of them with an error such that the modified datum is, say, as likely as not to be in excess or defect by 10 per cent.; is very unlikely to be out by 30 or 40 per cent.; and cannot, humanly speaking, be out by more than 50 per cent. A simple method of affecting a given set of figures with an error of this degree is to multiply each of them with a figure formed by adding together twenty digits taken at random from mathematical tables or statistical returns; dividing each product by 90 (the mean about which aggregates of twenty random digits hover). The data thus artificially affected with error are now to be used in the computation of an index-number, an erroneous number, which is to be compared with the result assumed to be true as having been deduced from the unfalsified data. A great number of such trials having been made, it will appear that the erroneous index-numbers deviate from the true one with the frequency and to the extent predicted by theory.

A specimen of this verificatory process is given on p. 318. The data employed by Mr. Palgrave in his computation of an index-number for 1885 [1] are assumed to be correct; then each datum is displaced or falsified in the manner above described, and a new (erroneous) index-number is deduced.

In this table the first column contains the names of articles in the order adopted by Mr. Palgrave in his Table 27. The second column contains the "weights" assigned by him under the heading of "Relative Importance." The third column consists of multipliers formed by adding twenty digits at random, and thus calculated to deflect the weights from their respective true values to the extent of, say, 12 per cent. on an average. The fourth column gives the new system of weights thus affected with error. The fifth column contains (comparative) prices taken from Mr. Palgrave's Table 26 for the year 1885. The sixth column furnishes a new set of multipliers assigned by chance. The seventh column gives the prices affected by error, and multiplied by 90 (the average value of the chance-multipliers). The eighth column gives the product of the erroneous weights and the erroneous prices (×90). The sum of this last column, 1,413,470,000, divided by ninety times the sum of the erroneous

[1] See p. 318.

weights, which sum is 172,486, gives the erroneous index-number 81; whereas the true index-number, on the assumption here made that Mr. Palgrave's data are absolutely correct, is, as computed by him, 76.[1]

Thus the falsified result is too great by $\frac{5}{76}$, or about 6 or 7 per cent. That is a result quite consonant with the theory which assigns such a *measure* of the error to be expected [2] that the result is as likely as not to be out by 4 per cent., and that the odds are only five to one against the error being so large as 8 or 9 per cent.

| Articles. | Real weights. | Sums of twenty digits. | Apparent weights. | Real prices. | Sums of twenty digits. | Apparent prices × 90. | Apparent weights × apparent prices, 0,000's omitted. |
|---|---|---|---|---|---|---|---|
| Cotton | 263 | 81 | 18903 | 38 | 82 | 3316 | 6268 |
| Silk | 12 | 69 | 828 | 53 | 99 | 5247 | 434 |
| Flax, etc. | 49 | 97 | 4753 | 61 | 97 | 5917 | 2812 |
| Wool | 142 | 80 | 11360 | 69 | 81 | 5589 | 6349 |
| Meat | 524 | 68 | 35632 | 102 | 74 | 7548 | 26913 |
| Iron | 150 | 81 | 12150 | 82 | 88 | 7216 | 8767 |
| Copper | 39 | 87 | 3393 | 59 | 87 | 5133 | 1739 |
| Lead | 13 | 66 | 858 | 57 | 85 | 4845 | 415 |
| Tin | 15 | 85 | 1275 | 78 | 104 | 8112 | 1034 |
| Timber | 164 | 71 | 11644 | 107 | 110 | 11770 | 13705 |
| Tallow | 28 | 87 | 2436 | 84 | 94 | 7896 | 1924 |
| Leather, etc. | 80 | 110 | 8800 | 110 | 84 | 9240 | 8131 |
| Indigo | 5 | 74 | 370 | 111 | 110 | 12210 | 4518 |
| Oils | 49 | 89 | 4361 | 69 | 69 | 4761 | 20805 |
| Coffee | 8 | 85 | 680 | 62 | 62 | 3844 | 2613 |
| Sugar | 149 | 80 | 11920 | 53 | 109 | 5777 | 6886 |
| Tea | 71 | 96 | 6816 | 69 | 79 | 5451 | 3438 |
| Tobacco | 29 | 93 | 2697 | 103 | 89 | 9167 | 2478 |
| Wheat | 410 | 81 | 33210 | 60 | 111 | 6660 | 22118 |
| Sums | — | — | 172086 | 1427 | 1714 | 129699 | 141347 |
| | — | — | — | 75·1 | — | 75·7 | 81 |

It would have been nothing miraculous if the result had been out by *sixteen* per cent.; nothing more extraordinary than, for instance, the fortuitous sequence which may be observed in our third column of *eight* random aggregates falling below the average about which they should oscillate, namely, 90.[3]

The same table furnishes another verification, if, making

[1] *Third Report on Depression of Trade,* Appendix B. Memorandum by R. I. Palgrave. Tables 26 and 27.

[2] Taking 8·5 as the *Modulus* of the resultant error. See above, p. 315.

[3] The probability of an error exceeding 1·9 times its modulus is ·0072. The probability of the sequence referred to is ·0078 $\left(=\frac{1}{2^7}\right)$.

abstraction of Mr. Palgrave's weights, we assume the index-number calculated on the principle of the economist to be correct, and regard the figures in our sixth column as erroneous weights (the true weights being all equal). Upon this understanding we have the true result, the Simple Arithmetic Mean of the comparative prices, 75·1; whereas the erroneously Weighted Mean is 75·7, that is, it is in excess by about ·8 per cent. Now the measure of error here predicted by theory[1] is such that an error of ·7 per cent. is as likely as not to occur. The occurrence of ·8 per cent. is therefore eminently consonant with the theory.[2]

It might be desirable to apply this sort of test on a large scale to the computation recommended by the Committee, and to prove by specific experience the conclusions which are deducible from the Theory of Probabilities concerning the accuracy of any index-number.

These conclusions cannot be stated in their most exact form until the price-returns, as well as the weights which enter into the computation to be tested, are assigned. But even at the present stage of our procedure, and without reference to the price-returns of a particular year, we may approximately estimate the accuracy of index-numbers of the kind proposed by the Committee. For the purpose of a rough estimate it is enough to know the weights (which are assigned in the Second Report of the Committee) and to utilise past experience concerning the course of prices in this country. A certain datum,[3] which had better be determined precisely from the price-returns from the particular year to which the index-number relates, may be approximately obtained by induction from the experience of past years.

Eliciting the required datum from the prices recorded by the *Economist*,[4] we may provisionally assert the following propositions concerning the accuracy of index-numbers such as the Committee

[1] By case (1) above, p. 306, the modulus is $\frac{1}{\sqrt{n}} \times \sqrt{\frac{\mathrm{SE}_r^2}{n}} \times \kappa$. Here $n$ is 19; $\frac{\mathrm{SE}_r^2}{n}$ is found to be ·08, and $\kappa$ is ·21. Whence the modulus is about ·014, or 1·5 per cent.

[2] Perhaps it may be asked here whether the example given is suited to exemplify our estimate of the *third* species of error (see above, p. 306): that due to the total omission of certain articles. The answer is that this estimate, involving a larger element of induction, does not profess to be so amenable to verification as those which are derived from known and steady "sources of error," like our aggregates of digits. Moreover, such verification as the theory admits would require a larger number of items than the table in the text contains.

[3] The coefficient C defined above, p. 308.

[4] As given in Mr. Palgrave's Table 26 (see above, p. 313).

has proposed. These, it will be recollected, involve twenty-seven English price-returns and twenty-seven assigned weights.[1]

(1) In such an index-number, if the weights alone are supposed subject to error, then the average error of the result, its erroneousness as one may say, is *twenty* times less than the error to which each weight is liable.

(2) If the price-returns alone are liable to error, the erroneousness of the result is about *four and a half* times less than that of each datum.

(3) In the general case, when both prices and weights are liable to error, then, if that error be the same for both species of data, the error of the result is still about four and a half times less than that same. If the error of the weights become twice as great as that which is incident to the prices, other things being the same, the error of the result is not materially increased. The error of the weights would need to be *five* times as great as that of the prices in order to increase the error of the result by 50 per cent. (making it only *three* times less than the error incident to the prices alone).

The practical conclusion from these propositions appears to be : Take more care about the prices than the weights.

More detailed statements cannot be made without some assumption as to the degree of inaccuracy to which our data are liable, the extent to which our estimates of weights and prices deviate from the figures which would be assigned if our knowledge and theory were perfect. In entertaining any suppositions as to the extent of this discrepancy, it is proper to conceive that the larger deviations, the more extensive errors, are less frequent in the long run, or more improbable. Thus, if we suppose that a deviation of each datum, weight or price, to the extent of 10 per cent. is as likely as not, then it may be presumed that a deviation of 20 per cent. is not likely, of 30 per cent. very unlikely. Upon this hypothesis, according to the general formulæ above investigated, the error, or fortuitous deviation from the ideal, to which the Committee's index-number is liable is as likely as not to be as large as 2 or 3 per cent., but is unlikely to be 6 per cent., and very unlikely to be 10 per cent. Now let us entertain the more unfavourable and almost certainly extravagant hypothesis that each datum is as likely as not to be out by 25 per

[1] Namely, 5, 5, 5, 5; 10, $2\frac{1}{2}$, $7\frac{1}{2}$; $2\frac{1}{2}$, $2\frac{1}{2}$, 9, $2\frac{1}{2}$, 1, $2\frac{1}{2}$; $2\frac{1}{2}$, $2\frac{1}{2}$, $2\frac{1}{2}$, $2\frac{1}{2}$; 10, 5, $2\frac{1}{2}$, $2\frac{1}{2}$; 3, 1, 1, 3, 1, 1. Whence the value of $\frac{Sw_r^2}{(Sw)^2}$ (see above, p. 310) is found to be ·05.

cent., and may just possibly err to the extent of cent. per cent. (an error which, if possible *in excess*, is almost inconceivable *in defect*). Upon this hypothesis our index-number is as likely as not to be out 5 per cent. but is not likely to be out by 10, and very unlikely to be out by 15, per cent.

The presumption that our calculation is not likely to be far out is confirmed by comparing the results obtainable by our method with those obtained by other operators upon different principles. If the compared figures differ little from each other it is presumable that they differ little from the true, the ideally best, figure: that which would be obtained if the data were perfect.

The index-numbers which challenge comparison with those proposed by the Committee may be arranged under four categories, namely:

I. Those which are formed by taking the *Simple Arithmetical Mean* of the given relative prices; the principle of the *Economist's* index-number, or rather what would be the principle of that operation if the prices operated on had not been selected with some reference to the quantity of the corresponding commodities.

II. What may be called the *Weighted Arithmetical Mean*, each relative price being affected with a factor proportioned to the quantity of the corresponding commodity, the principle adopted by the Committee.

III. The *Geometric Mean*, as employed by Jevons.

IV. The *Median*, proposed by the present writer as appropriate to certain purposes.[1] It is (in its simplest variety) formed by arranging the given price-variation (*e.g.*, 98, 80, 88, 87, 85) in the order of magnitude (*e.g.*, 80, 85, 87, 88, 98) and taking as the Mean the *middle* figure (in the above example the *third* figure, *i.e.*, 87).

Under each of these headings it is desirable to supplement actual verification with *a priori* reasoning based on the principles laid down in the earlier part of the Memorandum.

We may begin with the case (A) in which the comparative prices are supposed the same for the compared index-numbers. Later on (B) we shall take examples in which both the comparative prices and the mode of combining them are different.

## A.

I. Let us take the prices which are to hand for 21 (out of the 27) items of our index-number in Mr. Sauerbeck's well-known

[1] See Sect. IX. of the first Memorandum; above, p 247 *et seq.*

paper on the prices of commodities.[1] Let us form the Simple Arithmetic Mean of these prices for the year 1885, and compare it with the Mean obtained by applying our system of weights to the same prices. The operation is exhibited in the annexed table,

| | 1885. | | | 1873. | | |
|---|---|---|---|---|---|---|
| 1 | 2 | 3 | 4 | 5 | 6 | 7 |
| Articles common to Sauerbeck and the Committee. | Comparative Prices for 1885 given by Sauerbeck. | Weights assigned by the Committee. | Product of columns 2 and 3. | Comparative Prices for 1873 given by Sauerbeck. | Weights assigned by the Committee. | Product of columns 6 and 7. |
| Wheat . . . | 60 | 5 | 300 | 108 | 5 | 540 |
| Barley . . . | 77 | 5 | 385 | 104 | 5 | 520 |
| Oats . . . | 79 | 5 | 395 | 98 | 5 | 490 |
| Potatoes and rice . | 67 | 5 | 335 | 116 | 5 | 580 |
| Meat . . . | 88 | 10 | 880 | 109 | 10 | 1090 |
| Butter . . . | 89 | 7½ | 668 | 98 | 7½ | 735 |
| Sugar . . . | 59 | 2½ | 147·5 | 101 | 2½ | 252·5 |
| Tea . . . | 64 | 2½ | 160 | 102 | 2½ | 255·5 |
| Cotton . . . | 62 | 2½ | 155 | 100 | 2½ | 250 |
| Wool . . . | 73 | 2½ | 182·5 | 118 | 2½ | 345 |
| Silk . . . | 55 | 2½ | 137·5 | 95 | 2½ | 237·5 |
| Leather . . | 94 | 2½ | 235·5 | 117 | 2½ | 292·5 |
| Coal . . . | 72 | 10 | 720 | 145 | 10 | 1450 |
| Iron . . . | 60 | 5 | 300 | 170 | 5 | 850 |
| Copper . . | 57 | 2½ | 142·5 | 112 | 2½ | 280 |
| Lead . . . | 57 | 2½ | 142·5 | 117 | 2½ | 292·5 |
| Timber . . . | 81 | 3 | 243 | 111 | 3 | 333 |
| Petroleum . . | 55 | 1 | 55 | 122 | 1 | 122 |
| Indigo . . . | 72 | 1 | 72 | 92 | 1 | 92 |
| Flax . . . | 73 | 3 | 219 | 97 | 3 | 291 |
| Palm oil . . | 77 | 1 | 77 | 97 | 1 | 97 |
| Sums . . | 1471 | 81·5 ) | 5952 | 2329 | 81·5 ) | 9395·5 |
| Means . . | 70 | | 70·6 | 110·4 | | 115 |

the latter columns of which present a similar comparison for the year 1873. The two results may thus be summed up :

| | 1885. | 1873. |
|---|---|---|
| Simple Arithmetic Mean . . . . . . | 70 | 110·5 |
| The Committee's Weighted Arithmetic Mean . | 70·6 | 115 |

[1] *Journal of the Statistical Society*, 1886.

The relation between these results is predictable by, and consilient with, the conclusions of *a priori* reasoning. Accordingly the inference that the deviation between the two computations is not likely to exceed a small percentage may safely be extended to adjacent cases.

It follows, from the principles laid down in the earlier part of this Memorandum, that the discrepancy to be expected between the two results depends on three circumstances: the number of items, the inequality of the relative prices, and the inequality of the weights. The measure or *modulus* of the discrepancy is, in our notation,

$$\frac{1}{\sqrt{2n}} \times C \times \chi,$$

where $n$ is 21; C is presumed (by a sufficient, but certainly not very copious, induction) to be from ·2 to ·3; and $\chi$ is found to be about ·9.[1]

It follows that of the observed discrepancies, ·6 and 5, one is, *a priori*, more likely than not to occur, and the other not unlikely. A rapidly increasing improbability attaches to the higher degrees of divergence.

Of course it must be understood that this theorem in Probabilities, this statement of what will occur in the long run, is based upon the supposition that the weights are distributed impartially among the comparative prices. But if throughout the whole run the largest weight is attached to the largest, or smallest, observation, then the fortuitous character of the phenomenon is impaired. In fact the "long run" of which the theory may be expected to

[1] See above, p. 313, where the present writer records the *Mean Square of Deviation* for the comparative prices of nineteen different articles (given by the *Economist*) in different years. The *Mean Square of Deviation* for the figures given by Mr. Sauerbeck seems to be much the same. Again, the writer has, with much the same result, ascertained (by the Galton-Quetelet method) the quartiles for a few groups of English prices, like those given by Jevons. For example, in the case of the thirty-nine figures of the prices for prime articles in 1860–62 comparative with 1845–50 (*Currency and Finance*, pp. 51, 52) the quartile (half the interval between the tenth and the thirtieth) proves to be 11, corresponding to a modulus of about 22 per cent. If, however, we take in all the 118 articles given on the same page the quartile is 17. The groups of thirty-nine on Jevons' page 44, so far as they have been examined, give much the same result as the thirty-nine on pages 51, 52. Jevons himself gives $2\frac{1}{2}$ as the "probable error" incident to the *Mean* of thirty-six relative prices (*Currency and Finance*, p. 157)—corresponding to a probable error of 15, a modulus of 30 for the *individual* price-return. Doubtless the dispersion may be expected to be greater the more distant the base. If precision could be expected, it would be proper to express the coefficient as a percentage of the mean relative prices at each date rather than of the initial price or basis [as Bowley has done in the important Memorandum mentioned above, p. 198].

be true is a series of heterogeneous index-numbers not of consecutive years. Some imperfection of the sort noticed is observable in the case of Mr. Palgrave's Weighted Mean compared with the corresponding Simple Arithmetic Mean. The enormous weights attached to the continually low-priced *Cotton* and the continually high-priced *Meat* seem to affect the Weighted Mean abnormally. To effect the comparison, we must not take the averages given in Mr. Palgrave's Table 26, but those which are obtained by omitting from that table the three items *Cotton Wool*, *Cotton Yarn*, and *Cotton Cloth*, which do not occur in the compared Table 27. The annexed comparison does not present the appearance of pure chance. The discrepancies are rather *less* in magnitude than the theory requires. For the modulus, as deduced from Mr. Palgrave's system of weights, proves to be about 8·5 per cent. of the Mean 80 or 90:[1] that is about 7, corresponding to a probable error of about 3·5. The set of differences above registered seems to range a little within the limits so defined.

| | 1870 | 1871 | 1872 | 1873 | 1874 | 1875 | 1876 | 1877 | 1878 | 1879 | 1880 | 1881 | 1882 | 1883 | 1884 | 1885 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Mr. Palgrave's Weighted Mean for 19 articles | 90 | 93 | 100 | 104 | 108 | 97 | 99 | 100 | 95 | 82 | 89 | 93 | 87 | 88 | 80 | 76 |
| The Simple Arithmetic Mean for the same articles | 94 | 95 | 102 | 107·5 | 107 | 92 | 99 | 101 | 93 | 82 | 93·5 | 86 | 89 | 85·5 | 81 | 75 |
| Excess of Arithmetic over Weighted Mean | +4 | +2 | +2 | +3·5 | −1 | −5 | 0 | +1 | −2 | 0 | +4·5 | −7 | +2 | −2·5 | +1 | −1 |

The reason is, doubtless, that the impartial sprinkling of the prices among the weights, presupposed by theory, is not fulfilled in fact. Had it happened that throughout the whole run all the largest weights had been attached to the articles whose prices were continually low, e.g., *cotton*, and (for the last few years at least) *silk* and *flax*, then the discrepancies (between the weighted and simple mean) would have been rather larger than theory predicts. Thus, for the year 1885 I make *silk* exchange weights with *meat*, and thus bring down the index-number to 64; a discrepancy from the Arithmetic Mean which, if continued—as it probably would be—from year to year, would be a little too great. Similarly, when *wheat* exchanges weight with *leather*, and *cotton* with *indigo*, the index-number works out to 92—a discrepancy of two moduli, which is much too large for a continuance.

This sort of abnormality is less likely to occur in the case of our scheme, where none of the weights are so large as some of Mr. Palgrave's. Still, before pressing the theory, it is proper to examine whether the larger weights—in our case those of *meat*,

[1] See end of last note.

*fish*, and *coal*—are, from year to year, coupled with extreme relative prices.[1]

Whenever law of this sort is discernible the doctrine of Chances hides its inferior light, which is serviceable only in the night of total ignorance. The pure theory of Probabilities must be taken *cum grano* when we are treating concrete problems. The relation between the mathematical reasoning and the numerical facts is very much the same as that which holds between the abstract theory of Economics and the actual industrial world—a varying and undefinable degree of consilience, exaggerated by pedants, ignored by the vulgar, and used by the wise.

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Relative prices for 1885. | Weights assigned by Sauerbeck. | Product of columns 1 and 2. | Relative prices for 1873. | Product of columns 2 and 4. |
| 60 | 11 | 660 | 108 | 1188 |
| 77 | 5·5 | 423 | 104 | 572 |
| 79 | 6 | 474 | 98 | 588 |
| 67 | 6 | 402 | 116 | 696 |
| 88 | 15·5 | 1364 | 109 | 1689·5 |
| 89 | 3 | 267 | 98 | 294 |
| 59 | 5·5 | 325 | 101 | 555·5 |
| 64 | 2 | 128 | 102 | 204 |
| 62 | 10 | 620 | 100 | 1000 |
| 73 | 7·5 | 537·5 | 118 | 885 |
| 55 | 1 | 55 | 95 | 95 |
| 94 | 8 | 752 | 117 | 936 |
| 72 | 13 | 936 | 145 | 1885 |
| 60 | 5 | 300 | 170 | 850 |
| 57 | 1 | 57 | 112 | 112 |
| 57 | 0·5 | 28·5 | 117 | 58·5 |
| 81 | 2 | 162 | 111 | 222 |
| 55 | 0·5 | 27·5 | 122 | 61 |
| 72 | 0·5 | 36 | 92 | 49 |
| 73 | 1 | 73 | 97 | 97 |
| 77 | 0·5 | 15 | 97 | 19 |
| Sum . . | 105 | 7642·5 | — | 12056·5 |
| Mean . . | — | 73 | — | 115 |

[1] The effect of large weights combined with high prices is strikingly shown in an index-number (attributed to Dr. Paasche) which is published in Conrad's *Jahrbücher*, Vol. XXIII. p. 171. There are twenty-two items, among which *Rye* obtains about thirty per cent. of the total weight, and the Cereals generally (between whose prices there is a certain solidarity) about seventy per cent. It is no wonder that in the year 1868, when the price of the Cereals was exceptionally high, the Weighted Mean should be 118, while the Simple Arithmetic Mean of the twenty-two comparative prices is only 104.

II. Next let us compare our result with that obtained by using some other system of weights, *e.g.*, Mr. Sauerbeck's. In the table on page 325, column 1 is the same as column 2 of the table on p. 322, containing Mr. Sauerbeck's prices. Column 2 gives Mr. Sauerbeck's weights (for 1885) reduced to percentages of the total weight assigned by him to the twenty-one articles which are common to him and the Committee. For example, 61 is the weight actually assigned by him to wheat. This, multiplied by 100, and divided by 559, the sum of all the weights assigned by him to the twenty-one articles, gives 11 (nearly).

The comparison between the two systems is presented in the accompanying summary. The slightness of the difference between the compared results might have been predicted by theory, and may be predicted safely of adjacent cases.

| | 1885. | 1873. |
|---|---|---|
| The Committee's System of Weights . . . | 70·6 | 115 |
| Mr. Sauerbeck's System of Weights . . . | 73 | 115 |

III. We come next to the index-number of Jevons: the Geometric Mean of the relative prices appertaining to a number of groups. The definition of these groups is not wholly irrespective of their importance to the consumer and producer. There is evinced more or less concern that each article of equal importance should "count for one" in the composition of the index-number. But Jevons does not affect precision of weight. *Pepper*, for instance, forms one of the constituent thirty-nine articles.[1]

The analogue of this operation for our materials appears to be the Simple Geometric Mean of the relative prices for each of the articles specified in our scheme; except, indeed, those to which a very small weight, namely 1, has been assigned. Accordingly *Petroleum*, *Indigo*, *Palm Oil*, and *Caoutchouc* may, with propriety, be lumped into one group, for which the mean relative price is to be ascertained geometrically. For the sake of comparison with Mr. Sauerbeck's result Caoutchouc (not recorded by him) may be omitted from this little group. The Mean of the group so constituted is to be placed along with the relative prices

[1] In the "Serious Fall," republished in *Currency and Finance*, p. 44. In the "Variation of Prices" (*ibid.*, p. 142) Jevons seems to have employed the practice of weighting rather more extensively. He says, "Several qualities of one commodity have been joined and averaged before being thrown as one unit into larger groups"—in the case of certain articles which are not very clearly indicated. For the period after 1844 the [unweighted] "average prices, as calculated from the price-lists of the *Economist* . . . were mostly used."

for the remaining eighteen articles common to us and Mr. Sauerbeck, and the Geometric Mean of all the nineteen is to be taken. It proves to be 69 presenting the comparison herewith exhibited.[1]

| | |
|---|---|
| The Committee's Weighted Mean of 21 articles . . . . | 70·6 |
| The slightly adjusted Geometrical Mean of the same . . . | 69 |

The slightness of this divergence is conformable to theory. For it has been shown that the Weighted Mean (of twenty-one articles) is not likely to differ very much from the Simple Arithmetic Mean of the same. And it may be shown that the Arithmetic Mean is not likely to differ very much from the Geometric when the number of price-observations is large, and if they are not very unequal. This proposition may be illustrated by the following figures, the first row of which is obtained by taking the Arithmetic Mean of the thirty-nine price-percentages given—by Jevons in his paper on a "Serious Fall," etc. (*Currency and Finance*, p. 44). The second row consists of the Geometric Means, as given by him at p. 46, for the same figures. The superior magnitude of the Arithmetic Mean will be noticed. This circumstance (which Jevons thought an advantage on the side of his procedure) could not be predicated of a *Weighted* Arithmetic Mean (such as our index-number), as compared with the Geometric :—

| | 1851. | 1852. | 1853. | 1855. | 1857. | 1859. |
|---|---|---|---|---|---|---|
| Geometric Mean for 39 articles . | 92·4 | 93·8 | 111·3 | 117·6 | 128·8 | 116 |
| Arithmetic Mean for same . . | 94·6 | 94·6 | 112·4 | 119 | 134 | 119 |

IV. We come now to the *Median*, which has been recommended by the present writer as the formula for the most objective sort of

| Below 70. | Between 70 and 80. | Above 80. |
|---|---|---|
| . . .<br>. . .<br>. . .<br>. | 72 72 73 73 | . . .<br>. . .<br>. |
| Ten below 70 | Median = 72 | Seven above 80 |

[1] If we lump together *Barley* and *Oats* into one group, *Sugar* and *Tea* into another, and again *Copper* and *Lead*, the Geometric Mean of the sixteen returns thus presented is 70·2.

Mean between prices, not directed to any special purpose, such as the wants of the consumer or the difficulties of the producer, but more impersonal and absolute.

Of the twenty-one relative prices for 1885 given in the Table on p. 325 we have to take that which is the *eleventh* in the order of magnitude. To ascertain this we need not arrange *all* the figures in order. Having an inkling that the Mean is between 70 and 80, we shall find it sufficient to note the number of returns which lie outside those limits, and to write down in the order of magnitude only the returns which lie between 70 and 80. Thus, running our eye down the column of figures, we make a dot on the right for every return which is greater than 80, on the left for every one less than 70; and write down in the central compartment the figures which lie between 70 and 80 inclusive. Whence it appears that 72 is the figure eleventh in the order of magnitude: that is the Median.

| 1 | 2 | 3 |
|---|---|---|
| Comparative Prices. | Precisions determined by mass. | Arbitrary precisions. |
| 60 | 2 | 2 |
| 67 | 2 | 1 |
| 59 | 1·5 | 2 |
| 64 | 1·5 | 2 |
| 62 | 1·5 | 1 |
| 55 | 1·5 | 1 |
| 60 | 2 | 1 |
| 57 | 1·5 | 2 |
| 57 | 1·5 | 1 |
| 55 | 1 | 2 |
| | **16** | **15** |
| 72 | 3 | 2 |
| 72 | 1 | 1 |
| 73 | 1·5 | 1 |
| 73 | 1·5 | 1 |
| 77 | 1 | 1 |
| 77 | 2 | 2 |
| 79 | 2 | 1 |
| | **12** | **9** |
| 88 | 3 | 2 |
| 99 | 2·5 | 1 |
| 94 | 1·5 | 1 |
| 81 | 1·5 | 2 |
| | **8·5** | **6** |
| | **36·5** | **30** |

This is the Simple or Unweighted Median. There is a variety constituted by assigning special importance to those returns which we have reason to suppose are specially good representatives of the changes affecting the value of money. If, as in the writer's Memorandum often referred to,[1] we take mass * of commodity as the principle of ponderation, we shall have to proceed as follows with our twenty-one articles :—

As before, make three compartments for returns below 70, for those between 70 and 80, and for those above 80 respectively. Write down in the first and third compartments the returns in the order in which they occur (in any order); but in the central compartment in the order of magnitude.[2] In the second column of each compartment write the figures representing the relative *precision* assigned to each return. If these estimates of precision are based upon the quantities of commodity, it is recommended that they should be equal to, or rather less than, the square roots of the proportionate masses. Accordingly 2 has been put for the square root of 5, 1·5 for the square root of $2\frac{1}{2}$, and so on. Add together the sums of all the second columns. Thus, $16 + 12 + 8{\cdot}5 = 36{\cdot}5$. Find the central figure of the total second column : that is the figure which as nearly as may be has 18·25 for the sum of figures above it and below it. This figure proves to be the 3 at the top of the second compartment opposite 72. Then 72 is the required Mean.

In the third column another system of precisions has been tried to illustrate the effect of treating some relative prices as more typical of the change in the value of money than others. Tossing up a coin, the writer has stuck down (corresponding to each figure in the first column) 2 if heads turned up, 1 if tails. The sum of these arbitrary coefficients of precision is 30, and accordingly the adjusted Median is the point intermediate between 72 and the return next below in the order of magnitude, which proves to be 67. The adjusted Median is, therefore, 69·5.

By operating similarly on the price-returns for 1873 (given above) it is found that the Simple Median is 108, the Median adjusted by taking account of quantities still 108.

The deviation between the Median and the Simple (or other) Arithmetic Mean cannot, so far as the writer knows, be formulated

[1] Section IX. of the first Memorandum.

* "Commodity" may be understood as equivalent to "utility," when the term mass of commodity is put for volume of value representing quantity of satisfaction.

[2] It will probably be convenient to write these returns first in the order of their occurrence, and then rearrange them.

exactly. It diminishes with the number of observations, being of the order $\frac{1}{\sqrt{n}}$. A superior limit is given by a small multiple, say twice, $\sqrt{1 + \frac{1}{2}\pi}$ of Modulus of the observation; in our case of say ·1, or 10 per cent.[1] This limit is probably very superior, as the following trials, in addition to those given above, suggest :—

| | 1851. | 1852. | 1853. | 1855. | 1857. | 1859. |
|---|---|---|---|---|---|---|
| Arithmetic Mean for 39 articles . | 94·6 | 94·6 | 112·4 | 119 | 134 | 119 |
| Median for the same . . | 92 | 92 | 108 | 111 | 127 | 116·5 |
| Geometric Mean for the same . | 92·4 | 93·8 | 111·3 | 117·6 | 128·8 | 116 |

The thirty-nine figures are those above referred to, given by Jevons at p. 44 of his *Currency and Finance.* The Geometric Means have been cited again here in order to bring out the interest-

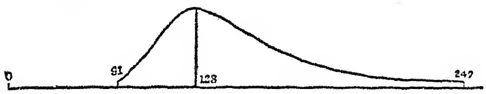

Fig. 1.

ing fact that the Median seems to keep closer to the Geometric than the Arithmetic. This property (which it would be desirable to verify more fully) is agreeable to the theory, first advanced by the present writer so far as he is aware, that prices are apt to group themselves in an unsymmetrical fashion after the pattern of the curve in Figure 1, whose ordinates indicate the frequency of each relative price. In the year 1857, for instance, the smallest figure was 91, the largest 247; while the Geometric, Median, and Arithmetic Means were respectively 129, 127, and 134. There is some reason to believe that the Geometric and Median—especially the latter—are more apt to be coincident with the point at which the greatest number of returns cluster, the greatest ordinate of the curve.

If then we take as our quæsitum *that figure* which *would be presented by the greatest number of relative prices* in the complete series of returns for all articles great and small, then, regarding our twenty-one, or it may be forty-five, articles as *specimens* of this series, we shall best operate on them by taking their Median.

And, even if this reasoning is not accepted, if the asymmetry

[1] See the writer's paper in "Problems in Probabilities," *Phil. Mag.*, Oct. 1886.

of the price-curve should not be regarded as serious, and the central point of the supposed symmetrical complete curve or series be taken as the quæsitum, still, even upon this hypothesis, the Median would have special claims.[1]

Another advantage—or the same otherwise viewed—on the side of the Median is its insensibility to accidental alterations of "weight." You may considerably increase or lighten the weights without causing this Mean to be depressed or elated. In the Arithmetic Mean a large weight happening to concur with an extreme relative price produces a derangement which with reference to the present objective[2] (as distinguished from the "consumption") standard may be regarded as accidental. The Median is free from this fortuitous disturbance. The rationale of this stability is supplied by the Calculus of Probabilities.

It appears, therefore, that our index-number, though not likely to be wide of any mark which has been proposed, is not the one which is most accurately directed to a particular, or rather, indeed, the most general object. It is no matter of surprise or complaint that we should not hit full in the centre an object which has not been our aim; our index-number being mainly a *Standard of Desiderata*, measuring the variation in value of the national consumption. Our primary aim, indeed, is more comprehensive, not this special, but a collective, or "compromise," scope; not so much to hit a particular bird, but so to shoot among the closely clustered covey as to bring down most game. But then we are brought back to, or nearly to, the directer aim and simpler object by a consideration which has great weight in practical economics, the necessity of adopting a principle—as Mill says with respect to convertible currency—"intelligible to the most untaught capacity." Now every tyro in our subject makes straight for the Consumption Standard; but the more delicate distinctions of the Producers' Standard and the typical or quasi-objective index-number evade popular perception.

In view of this practical exigency it may well be that the Committee's index-number is the one best adapted to purposes in general—the principal standard as defined in the First Report.

[1] The problem would then be analogous to the reduction of symmetrical observations relating to a physical quantity. On account of the "discordance" of the price-observations, their very different liability to fluctuation, the writer would recommend the use of the Median on the grounds which he has stated in the paper on "Discordant Observations," *Phil. Mag.*, April 1886.

[2] See the first Memorandum (H), and also *Journal of the Statistical Society*, June 1888.

What is here contended is that, with respect to a certain purpose other than the consumers' interest, the Committee's index-number is on the one hand likely to be a very good measure, and on the other hand not the very best possible.*

## B.

We have now to compare index-numbers differing as to the prices operated on as well as the methods of operation. One important case is where the prices of the principal articles are the same for the compared index-numbers, the data differing only as to a small part of the total value. For example, of the total value covered by Mr. Sauerbeck's index-number about $\frac{9}{10}$ is common to the Committee's scheme. For Mr. Sauerbeck's weights (or " nominal values ") of the twenty-one articles common to both calculations make up (for the year 1885) 559, while the sum for all the items treated by him is 617.

Let us see then what difference is caused by operating on all Mr. Sauerbeck's forty-five articles instead of only the twenty-one principal items which are common to his price list and ours. He himself (at p. 595 of the *Journal of the Royal Statistical Society*, 1886) gives us the Means of Comparison :—

| | 1885. | 1873. |
|---|---|---|
| Mr. Sauerbeck's Weighted Mean for 45 articles . . | 71·2 | 115·2 |
| The Committee's Weighted Mean for 21 of those articles | 70·6 | 115 |

It is interesting to observe that the Median does not suffer any change by being extended from twenty-one to forty-five articles. The attention of the reader is invited also to the ease

| Above 80. | Between 70 and 80. | | | | | | | | | | | | | | Below 80. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Dots to the number of 12. | 79 | 78 | 77 | 77 | 76 | 75 | 75 | 73 | 73 | 73 | 72 | 72 | 71 | 70 | Dots to the number of 19. |

of this method. In order to take in the twenty-four additional articles we have only to write down a few more figures in the central compartment, to add a few more dots in the extreme compartments, as shown in the annexed diagram. Indeed it is not necessary to record the number of observations (by way of

* Compare the remarks at the end of the first Memorandum above, p. 256.

| 1 | 2 | 3 | 1 (*continued*). | 2 (*continued*). | 3 (*continued*). |
|---|---|---|---|---|---|
| Relative prices. | Precision. | Another System of Precision. | Relative prices. | Precision. | Another System of Precision. |
| 60 | 2 | | 72 | 1 | 3 |
| 62 | 3 | 3 | 73 | 2 | 1 |
| 63 | 3 | 3 | 73 | 2 | 2 |
| 64 | 2 | 3 | 73 | 1 | 3 |
| 59 | 1 | 2 | 75 | 1 | 1 |
| 65 | 2 | 2 | 75 | 2 | 2 |
| 58 | 3 | 1 | 76 | 1 | 1 |
| 64 | 3 | 2 | 77 | 3 | 2 |
| 50 | 2 | 2 | 77 | 2 | 3 |
| 55 | 1 | 1 | 79 | 1 | 2 |
| 55 | 2 | 1 | | 1 | 2 |
| 60 | 2 | 3 | | 3 | 1 |
| 59 | 2 | 3 | | 3 | 3 |
| 57 | 1 | 2 | | 2 | 3 |
| 57 | 1 | 3 | | 2 | 3 |
| 62 | 2 | 2 | | 1 | 3 |
| 63 | 1 | 2 | | 3 | 2 |
| 63 | 2 | 2 | | 3 | 2 |
| | | 1 | | 3 | 3 |
| 70 | 3 | 3 | | 1 | 1 |
| 70 | 2 | 1 | | 2 | 1 |
| 71 | 2 | 2 | | 2 | 1 |
| 72 | 1 | 3 | | 1 | 2 |
| Sums . | 43 | 47 | — | 43 | 47 |

dots) in more than *one* of the extreme compartments. The Median is the *twenty-third* figure in the order of magnitude, that is, 72. Proceeding similarly for the year 1873, we find the Median of Mr. Sauerbeck's forty-five relative prices 109.

Now let us try the effect of weighting. Running my eye over some pages of statistics, I assign the digits 1, 2, 3 as they occur to the comparative prices, which are in pell-mell order up to 70; between 70 and 80 in the order of magnitude; and above 80 are not represented at all. The sum of the whole second column thus formed is 86. The central point corresponding to half that sum is at the foot of the first half of the second column, corresponding to the entry 72 in the first column. Accordingly 72 is the adjusted Median. I try another system of precision-factors arbitrarily assigned. And still the Median is 72!

The comparisons offered by Mr. Sauerbeck's materials are summed up in the table on p. 334.

For estimating the extent of difference to be expected between two index-numbers which overlap as to some of their items, a formula is derivable from the above reasoning. Of course, as the number of items common to two compared index-numbers is diminished the chances of their dissilience are increased. The

| | 1885. | | 1873. | |
|---|---|---|---|---|
| | The 21 articles common to the Committee and Sauerbeck. | Sauerbeck's 45 articles. | The 21 articles common to the Committee and Sauerbeck. | Sauerbeck's 45 articles. |
| The Simple Arithmetic Mean . | 70 | 74 | 110·5 | 111 |
| The Committee's Weighted Mean . | 70·6 | — | 115 | — |
| Sauerbeck's Weighted Mean . . | 73 | 72·5 | 115 | 115·2 |
| Jevons' adjusted Geometric Mean . | 69 | — | — | — |
| The Simple Median . . . | 72 | 72 | 108 | 109 |
| The Median adjusted according to *quantity* | 72 | — | 108 | — |
| The Median adjusted on an arbitrary principle . . . | 69·5 | 72 | — | — |

*art of conjecturing* can in such a case throw only a very feeble light on the relation between two such index-numbers. For instance, it could hardly have been predicted that the Simple Arithmetic Mean for Mr. Sauerbeck's forty-five articles should differ so little as ·5 from the same Mean for twenty-one articles, as proved to be the case for the year 1873. It is even more surprising that if for 1885 we complete our index-number, taking account of the six items belonging to our scheme not included by Mr. Sauerbeck, there is a marked rise in the index-number owing to all these six returns being above the average. The following little table is formed by comparing the prices in 1885 with the average for 1866–77 as given in the *Statistical Abstract* :—

| Articles omitted hitherto. | Relative prices for 1885. | Weight assigned by the Committee. | Product of Columns 2 and 3. |
|---|---|---|---|
| 1 | 2 | 3 | 4 |
| Fish [1] . . . . | 104 | 2½ | 260 |
| Beer . . . . | 76 | 9 | 68 |
| Spirits [2]. . . . | 120 | 2½ | 300 |
| Wine . . . . | 100 | 1 | 100 |
| Tobacco . . . | 85 | 2½ | 212½ |
| Caoutchouc . . . | 109 | 1 | 109 |
| Sums . . . . | 594 | 18·5 | 1665 |
| Means . . . . | 97 | — | 90 |

[1] Fish imported. [2] Spirits other than rum and brandy.

If we add the outcome of this table to that of the first table representing the other twenty-one articles, we have $1665 + 5952 = 7617$; which, divided by 100, gives the new index-number 76.

Of course in applying the doctrine of Chances to this problem we must abstract all *animus*. If you pick out the large relative prices and the large weights you will doubtless succeed, like Mr. Forsell, in producing discrepancies—though even his success in that attempt seems less than might have been expected.

In concluding this comparison of results the writer may say, in the phrase of Jevons, that he has taken more than reasonable pains to secure arithmetical accuracy. No doubt mistakes will have come. But, as the calculations have been performed without any conscious bias, it may be hoped that the errors will neutralise each other, and that the general impression left by the work is correct.

# APPENDIX.

## Statement of the Extent, and Estimate of the Significance, of the Difference between the Committee's Scheme and others.

Table I.

| Articles common to the Committee and Mr. Sauerbeck's Weighted Index-number. | Weights actually assigned. | | Weights as Percentages of Total Weight of the Common Articles. | | Differences between Columns 4 and 5. |
|---|---|---|---|---|---|
| | The Committee. | Mr. Sauerbeck. | The Committee. | Mr. Sauerbeck. | |
| 1 | 2 | 3 | 4 | 5 | 6 |
| Wheat | 5 | 61 | 6 | 11 | 5 |
| Barley | 5 | 30 | 6 | 5·5 | ·5 |
| Oats | 5 | 32 | 6 | 6 | 0 |
| Potatoes and rice | 5 | 32 | 6 | 6 | 0 |
| Meat | 10 | 88 | 12 | 15·5 | 3·5 |
| Butter | 7½ | 23 | 9 | 3 | 6 |
| Sugar | 2½ | 30 | 3 | 5·5 | 2·5 |
| Tea | 2½ | 15 | 3 | 2 | 1 |
| Cotton | 2½ | | 3 | 10 | 7 |
| Wool | 2½ | 42 | 3 | 7·5 | 4·5 |
| Silk | 2½ | 4 | 3 | 1 | 2 |
| Leather | 2½ | 10 | 3 | 2 | 1 |
| Coal | 10 | 74 | 12 | 13 | 1 |
| Iron | 5 | 27 | 6 | 5 | 1 |
| Copper | 2½ | 7 | 3 | 1 | 2 |
| Lead | 2½ | 3 | 3 | ·5 | 2·5 |
| Timber | 3 | 17 | 4 | 2 | 2 |
| Petroleum | 1 | 3 | 1 | ·5 | ·5 |
| Indigo | 1 | 3 | 1 | ·5 | ·5 |
| Flax | 3 | 4·5 | 4 | 1 | 3 |
| Palm oil | 1 | 1·5 | 1 | 0 | 1 |
| Sums | 81·5 | 564 | 98 | 98·5 | 46·5 |

TABLE II.

| Articles common to the Committee and Mr. Palgrave. | Weights as Percentages of Total Weight of the Common Articles. | | Differences between Columns 2 and 3. |
|---|---|---|---|
| | The Committee. | Mr. Palgrave. | |
| 1 | 2 | 3 | 4 |
| Wheat . . . . . | 10 | 19 | 9 |
| Meat . . . . . | 20 | 25 | 5 |
| Sugar . . . . . | 5 | 7 | 2 |
| Tea . . . . . | 5 | 3·5 | 1·5 |
| Tobacco . . . . | 5 | 1 | 4 |
| Cotton . . . . . | 5 | 12 | 7 |
| Wool . . . . . | 5 | 6·5 | 1·5 |
| Silk . . . . . | 5 | ·5 | 4·5 |
| Leather . . . . . | 5 | 3·5 | 1·5 |
| Iron . . . . . | 10 | 7 | 3 |
| Copper . . . . . | 5 | 1·5 | 3·5 |
| Lead . . . . . | 5 | ·5 | 4·5 |
| Timber . . . . . | 6 | 7·5 | 1·5 |
| Indigo . . . . . | 2 | 0 | 2 |
| Flax . . . . . | 6 | 2 | 4 |
| Oil [1] . . . . . | 2 | 2 | 0 |
| Sums . . . . | 101 | 98·5 | 54·5 |

[1] *Palm oil* in the Committee's scheme; *oils* in Mr. Palgrave's.

TABLE III.

| Articles common to the Committee and Mr. Sauerbeck's Unweighted Index-number. | Weights actually assigned. | | Weights as Percentages of Total Weight of the Common Articles. | | Differences between Columns 4 and 5. |
|---|---|---|---|---|---|
| | The Committee. | Mr. Sauerbeck. | The Committee. | Mr. Sauerbeck. | |
| 1 | 2 | 3 | 4 | 5 | 6 |
| Wheat . . . . . | 5 | 3 | 6 | 8·5 | 2·5 |
| Barley . . . . . | 5 | 1 | 6 | 3 | 3 |
| Oats . . . . . | 5 | 1 | 6 | 3 | 3 |
| Potatoes and rice . . | 5 | 2 | 6 | 6 | 0 |
| Meat . . . . . | 10 | 6 | 12·5 | 17 | 4·5 |
| Butter . . . . . | $7\frac{1}{2}$ | 1 | 9 | 3 | 6 |
| Sugar . . . . . | $2\frac{1}{2}$ | 2 | 3 | 6 | 3 |
| Tea . . . . . | $2\frac{1}{2}$ | 1 | 3 | 3 | 0 |
| Cotton . . . . . | $2\frac{1}{2}$ | 2 | 3 | 6 | 3 |
| Wool . . . . . | $2\frac{1}{2}$ | 2 | 3 | 6 | 3 |
| Silk . . . . . | $2\frac{1}{2}$ | 1 | 3 | 3 | 0 |
| Leather . . . . . | $2\frac{1}{2}$ | 2 | 3 | 6 | 3 |
| Coal . . . . . | 10 | 2 | 12·5 | 6 | 6·5 |
| Iron . . . . . | 5 | 2 | 6 | 6 | 0 |
| Copper . . . . . | $2\frac{1}{2}$ | 1 | 3 | 3 | 0 |
| Lead . . . . . | $2\frac{1}{2}$ | 1 | 3 | 3 | 0 |
| Timber . . . . . | 3 | 1 | 4 | 3 | 1 |
| Petroleum . . . . | 1 | 1 | 1 | 3 | 2 |
| Flax . . . . . | 3 | 1 | 4 | 3 | 1 |
| Indigo . . . . . | 1 | 1 | 1 | 3 | 2 |
| Palm oil . . . . | 1 | 1 | 1 | 3 | 2 |
| Sums . . . . | 81·5 | 35 | 99 | 103·5 | 45·5 |

TABLE IV.

| Articles common to the Committee and Dr. Soetbeer. | Weights actually assigned. | | Weights as Percentages of Total Weight of the Common Articles. | | Differences between Columns 2 and 5. |
|---|---|---|---|---|---|
| | The Committee. | Soetbeer. | The Committee. | Soetbeer. | |
| 1 | 2 | 3 | 4 | 5 | 6 |
| Wheat | 5 | 2 | Practically same as Column 2. | 4·5 | ·5 |
| Barley | 5 | 2 | | 4·5 | ·5 |
| Oats | 5 | 1 | | 2 | 3 |
| Potatoes and rice | 5 | 2 | | 4·5 | ·5 |
| Meat | 10 | 4 | | 9 | 1 |
| Fish | 2½ | 2 | | 4·5 | 2 |
| Butter, milk, and cheese | 7½ | 2 | | 4·5 | 3 |
| Sugar | 2½ | 2 | | 4·5 | 2 |
| Tea | 2½ | 1 | | 2 | ·5 |
| Beer | 9 | 1 | | 2 | 7 |
| Spirits | 2½ | 3 | | 7 | 4·5 |
| Wine | 1 [1] | 2 | | 4·5 | 3·5 |
| Tobacco | 2½ | 1 | | 2 | ·5 |
| Cotton | 2½ | 1 | | 2 | ·5 |
| Wool | 2½ | 1 | | 2 | ·5 |
| Silk | 2½ | 1 | | 2 | ·5 |
| Leather, etc. | 2½ | 3 | | 7 | 4·5 |
| Coal | 10 | 1 | | 2 | 8 |
| Iron | 5 | 3 | | 7 | 2 |
| Copper | 2½ | 1 | | 2 | ·5 |
| Lead | 2½ | 1 | | 2 | ·5 |
| Timber | 3 | 3 | | 7 | 4 |
| Indigo | 1 | 1 | | 2 | 1 |
| Flax | 3 | 1 | | 2 | 1 |
| Palm oil | 1 | 1 | | 2 | 1 |
| Sums | 98 | 43 | 98 | 94·5 | 52·5 |

[1] *Hops.*

TABLE V.

| Articles common to the Committee and Jevons. | Weights actually assigned. | | | Weights relative to the Total Weight of the Common Articles. | | | Differences between Columns 6 and 5. | Differences between Columns 7 and 5. |
|---|---|---|---|---|---|---|---|---|
| | The Committee. | Jevons. | | The Committee. | Jevons. | | | |
| | | $a$[1] | $b$[2] | | $a$[1] | $b$[2] | $a$[1] | $b$[2] |
| 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 |
| Wheat . . | 5 | 1 | 1 | 7 | 3·5 | 2 | 3·5 | 5 |
| Barley . . | 5 | 1 | 1 | 7 | 3·5 | 2 | 3·5 | 5 |
| Oats . . | 5 | 1 | 1 | 7 | 3·5 | 2 | 3·5 | 5 |
| Meat . . | 10 | 3 | 5 | 14·5 | 11 | 9 | 3·5 | 5·5 |
| Butter and cheese | $7\frac{1}{2}$ | 1 | 3 | 11 | 3·5 | 6 | 7·5 | 5 |
| Sugar . . | $2\frac{1}{2}$ | 1 | 3 | 4 | 3·5 | 5 | ·5 | 1 |
| Tea . . | $2\frac{1}{2}$ | 1 | 4 | 4 | 3·5 | 7 | ·5 | 3 |
| Spirits . . | $2\frac{1}{2}$ | 1 | 3 | 4 | 3·5 | 6 | ·5 | 2 |
| Cotton . . | $2\frac{1}{2}$ | 3 | 3 | 4 | 11 | 6 | 7 | 2 |
| Wool . . | $2\frac{1}{2}$ | 1 | 2 | 4 | 3·5 | 4 | ·5 | 0 |
| Silk . . | $2\frac{1}{2}$ | 1 | 3 | 4 | 3·5 | 6 | ·5 | 2 |
| Leather . . | $2\frac{1}{2}$ | 2 | 4 | 4 | 7 | 7 | 3 | 3 |
| Iron . . | 5 | 3 | 3 | 7 | 11 | 6 | 4 | 1 |
| Copper . . | $2\frac{1}{2}$ | 1 | 1 | 4 | 3·5 | 2 | ·5 | 2 |
| Lead . . | $2\frac{1}{2}$ | 1 | 4 | 4 | 3·5 | 7 | ·5 | 3 |
| Timber . . | 3 | 2 | 6 | 4·5 | 7 | 11 | 2·5 | 6·5 |
| Flax . . | 3 | 1 | 1 | 4·5 | 3·5 | 2 | 1 | 2·5 |
| Indigo . . | 1 | 1 | 1 | 1·5 | 3·5 | 2 | 2 | ·5 |
| Palm oil . . | 1 | 1 | 1 | 1·5 | 3·5 | 2 | 2 | ·5 |
| (Wine) . . | $(2\frac{1}{2})$ | — | 4 | (4) | — | 7 | — | 3 |
| Sums . | 68 | 27 | 54 | 101·5 | 96 | 101 | 45·5 | 57·5 |
| | (70·5) | — | — | (105·5) | — | — | — | — |

[1] First form of index-number based upon 39 articles ("Serious Fall").
[2] Second form of index-number based upon 118 articles (*ibid.*).

TABLE VI.

| Index-numbers compared with the Committee's. | Mr. Sauerbeck's Weighted. | Mr. Palgrave's. | Mr. Sauerbeck's Unweighted. | Dr. Soetbeer's. | Jevons. | |
|---|---|---|---|---|---|---|
| | | | | | *a* | *b* |
| Number of articles common to the Committee's and other index-numbers | 21 | 16 | 21 | 25 | 19 | 20 |
| Mean difference (per cent.) between the weights of the common articles according to the Committee's and other schemes | 50 | 54 | 45 | 53 | 45 | 58 |
| Weight of the common articles per cent. of the weight of all the articles in the Committee's scheme | 81·5 | 50·5 | 81·5 | 98 | 68 | 70·5 |
| Weight of the common articles per cent. of the weight of all the articles in other schemes | 90·5 | 98 | 78 | 44 | 70 | 54 |
| Discrepancy as likely as not to occur between the Committee's and other results | 2 | 2·5 | 2 | 2 | 2·5 | 2·5 |
| Discrepancy very unlikely to occur between the Committee's and other results | 8 | 11 | 8 | 8 | 10 | 11 |

*Remarks upon the preceding Tables.*

These tables present a comparison between the index-number proposed by the Committee and some other well-known constructions of the same kind. In the first five tables the feature of comparison consists of those articles or items which are common to the Committee's and the compared schemes. The tables show the different importance or "weight" assigned to the same items in the Committee's and each of the other schemes. For the purpose of exhibiting this difference it is proper to contrast, not the actual weights employed by the Committee and each compared index-number, but the weights relative to the total weight assigned to the common items by the Committee's and the compared scheme respectively. Thus, in the first table, the first column states the articles, twenty-one in number, which are common to the Committee's index-number and to one which has been given by Mr. Sauerbeck (*Journal Statistical Society*, 1886, p. 595). The second and third columns give the weights actually affixed by the Committee and Mr. Sauerbeck respectively to the comparative prices of those twenty-one articles. The third and fourth columns give the weights relative to the total weight of the coincident

portions of the two systems. Thus, 61 being the weight actually assigned by Mr. Sauerbeck to wheat, while 564 is the sum of the weights attached by him to all the articles common to him and the Committee, $\frac{61}{564}$, or the same fraction multiplied by 100 (= 11 nearly), is taken as the proper weight according to Mr. Sauerbeck for wheat; in a curtailed index-number covering only those articles common to him and the Committee. By parity $\frac{5}{81\cdot 5} \times 100$, or six nearly, is the weight for the same article according to the Committee. In the sixth column the differences—the absolute differences without regard to *sign*—between the respective weights are given. To appreciate the importance of this difference of weight, we must consider it in relation to the absolute (mean) weight. Thus $\frac{\text{Mean difference of weight}}{\text{Mean weight}}$ is the fraction (or, multiplied by 100, the percentage) which most, or at least very, properly measures the discrepancy between the two systems. Now the Mean weight for each of the two compared systems is $\frac{100}{21}$. Therefore we have for the required measure

$$\frac{\text{Sum of differences}}{21} \div \frac{100}{21}, \text{ or simply } \frac{\text{Sum of differences}}{100}$$

(or, expressed as a percentage, the sum of differences). Thus in the case before us the average deviation between the compared weights is 49·5, or 50 per cent. (nearly). This figure is useful as enabling us (taking into account the number of common items) to predict the extent of discrepancy which is likely to exist between the results of the two methods of treating the common data.

The second table presents a similar comparison between the Committee's and Mr. Palgrave's index-number ("Third Report of the Committee on Depression of Trade," Appendix B). It has not been thought necessary to record the actual weights. Those employed in the computation of the "relative" weights according to Mr. Palgrave were the figures of *comparative importance* given by him for the year 1885, which differ very little from the corresponding entries in previous years. The coefficient of discrepancy between the two results being much the same as in the former comparison, we may expect much the same difference, or rather one somewhat larger, since the number of common items (sixteen) is here somewhat smaller (than twenty-one).

The remaining index-numbers do not equally admit of being laid alongside that of the Committee for the purpose of comparison. They are as it were in a different plane, adopting a different formula (as well as different constants) from the Committee. In

these schemes, unlike the Committee's, each comparative price is not affected with a factor or weight corresponding to its importance. *Prima facie* every relative price counts for one; but the principle of weight is to some extent asserted by introducing as independent items several species belonging to one genus. Thus in Mr. Sauerbeck's *unweighted* index-number, our Table 3, there figure *two* species of *wheat* and also one of *flour*; in effect assigning a weight of *three* to *wheat*. There is indeed something arbitrary in such interpretation. For in comparing this sort of index-numbers with the Committee's it is hardly possible—as in the case of the explicitly weighted index-numbers—to suppose the prices (for the common articles) to be the *same* in the two compared calculations. For example, our price of wheat is taken from the "Gazette"; theirs may be a Mean of that price and the price of flour. Accordingly the estimate of the difference to be expected (proportioned to the total of the last column) is apt to be less accurate, to be under the mark, in these cases. A further inaccuracy affects this estimate in the case of Jevons' index-number, our Table 5, namely, that he adopted the *Geometrical* method of combining relative prices. In fact, our estimates apply only to the *Arithmetic* combination of Jevons' materials, to be supplemented by the observed fact that the Arithmetic and Geometric Means of prices do not much differ.

The last table resumes the results of the first five in its first and second rows. The first row states the number of items common to the Committee with each of the compared schemes—a necessary datum for the estimate of the discrepancy likely to exist between the results. *Ceteris paribus*, this discrepancy is *inversely* proportioned to the *square root* of the number of common items. The second row gives the mean difference between the respective weights as above defined. The third and fourth rows compare the Committee's index-number with each of the others as to the extent of the materials not common to both. The comparison may be thus illustrated. Let CO represent by its length the


quantity of weight common to the Committee and the other index-number. Let CC′ represent the total weight of all the articles in the Committee's system, and OO′ that of the other system. The third row gives the ratio of CO to CC′, and the fourth column the ratio of CO to OO′.

The last two rows give an estimate of the discrepancy likely or unlikely to occur between the results of the compared compu-

tations. This estimate involves (in addition to the data contained in the preceding rows) a constant or coefficient deduced from the course of English prices in past years : the inequality or *dispersion* of price-variations, which keeps pretty constant from year to year. The estimates are therefore only applicable to England. They are to be taken *cum grano,* with the reservations stated in various parts of the Memorandum.

## ( J )

## VARIORUM NOTES ON INDEX-NUMBERS

[In the following article, of which the original title was "Recent Writings on Index-numbers"—recent in 1894, in which year the article appeared in the Economic Journal—the properties of this method of measurement are further considered. There appear some grounds for hoping that the labour-standard (above, p. 195) may prove not to diverge widely from the more commonly adopted consumption or commodity standard. The reconciliation would be important in the eyes of those who hesitate to follow the guidance of Professor Irving Fisher and other leaders who show the way to monetary stability, because it is uncertain what is the proper conception of monetary stability, towards which of two distinct destinations our movement should be directed.

In accordance with my general practice, I have suppressed some controversial paragraphs in this paper. But here, as throughout this Collection, the necessity of making clear my meaning has prevailed over my aversion to controversy.]

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One of the problems which has exercised economists for some years, the determination of variations in the value of the monetary standard, bears some not wholly accidental resemblances to one of the problems which has exercised philosophers in all ages, the determination of the standard of moral action. With respect to both problems there are wise men who despair of determinateness; there are enthusiasts, of whom each is confident that he has obtained *the* solution. With respect to both problems the discrepancy in principle is greater than the difference in practice.

These reflections are illustrated by Dr. Lindsay's recent volume; which is mentioned here only as typical of the sort of intolerance which—in monetary as in ethical theory—is apt to characterise common sense. It is not denied that the author has made good use of sound methods.[1]

[1] *Die Preisbewegung der Edelmetalle seit,* 1850. Jena: G. Fischer, 1893 (ch. iii. *et passim*).

The strife between rival methods may be somewhat abated by considering the second report of the United States Finance Committee upon the course of prices and wages; the results of which are summed up by Professor Taussig in a masterly paper read before the International Statistical Institute at Chicago.[1] There is hardly any difference between the index-numbers for the course of prices since 1860, as determined by a simple average, or by one weighted according to the importance of each article to the consumer—an importance which was measured by the proportions in which different articles entered into the average "budget" or expenditure of families of small means. Professor Taussig says: "If these two methods of simple arithmetical average on the one hand and average weighted by family budget importance on the other hand yielded greatly different results, we might be perplexed which to use as significant of the general course of prices."

It must not be supposed that this sort of perplexity is always equally well avoided. There has lately been agitated a question of principle upon the answer to which depends a material difference in practice. Should the standard of deferred payments—the amount payable at future epochs to a creditor—be the product of a constant quantity of effort and sacrifice, the same "value" in Ricardo's terminology,[2] or a constant quantity of commodity, the same amount of "riches"?

Professor Simon Newcomb [3] goes so far as to say—

"The fundamental idea on which the tabular standard ["twenty years ago supposed to afford a satisfactory solution to the problem"] was based, was that human labour itself furnished the best possible standard."

In a similar spirit Mr. Leonard Courtney, in his candid article on Bimetallism, writes:— [4]

"We may aim not at a redelivery of article by article, but at a repayment of labour by labour or of sacrifice by sacrifice. . . . I do not stop to investigate the ethical foundation of this principle, which might lead us far afield; but I believe the standard so described does represent what would commonly be accepted as the *desideratum.*"

This standard derives some support from the argument

[1] Published in the *Yale Review* for November 1893.

[2] *Political Economy*, ch. xx.

[3] In his article in the September number of the *Journal of Political Economy*, Chicago, Vol. I. p. 505.

[4] In the *Nineteenth Century* for April 1893.

employed by Dr. L. S. Merriam in a recent article [1] that "the restoration of equal value or equal amounts of final utility"—a principle underlying approved standards—"means also the restoration of equal amounts of final disutility." [2]

Mr. E. A. Ross objects that as the goods restored would not all be employed at the margin of expenditure, the increase in the quantity of goods payable by the debtor should not be measured by the decrease in their *final* utility. This objection is valid against the exact correspondence between the labour standard and the utility standard which Dr. Merriam had suggested in virtue of the condition that final utility balances final disutility.[3] But Professor Ross does not disprove a rough correspondence between the utility standard as corrected by reference to total rather than final utility, and the disutility standard in the only form in which it is practically proposed to employ it—viz. assuming the total labour per head at the periods compared to be constant, and taking the ratio between the total quantity of goods produced per head now, and the corresponding total at a former epoch, as the measure of the increase in the quantity of goods produced by a unit of labour.[4] The depreciation of goods, if I may be allowed the expression, thus determined by the disutility standard may well coincide with—there is no reason why it should exceed—the depreciation determined by the (total) utility standard.

This possibility becomes fortified by the consideration that, as Mr. Ross well puts it, "the total well-being we derive from goods depends not only on the positive satisfaction experienced in use or consumption," but also "on the social satisfactions that flow to us in consequence, the latter largely determined by the relation of our consumption to that of our neighbours." [5] In a progressive state of society the second circumstance as well as the first tends to depreciate goods with respect to utility, and *pro tanto* increases the probability that the appreciation of money as measured by the corrected utility standard will not

[1] "The Standard of Deferred Payments." *Amer. Ac. Pol. Sci.*, January 1893. It is sad to learn that the promising author of this just and ingenious argument has been the victim of a boating accident.

[2] On the idea of the final utility of wealth decreasing with the progress of society see the first of the British Association Memoranda above referred to, H. p. 210.

[3] *Amer. Ac. Pol. Sci.*, November 1893.

[4] This argument may be illustrated by the use of diagrams such as Jevons has employed in his *Theory* to denote the total and final utility of consumption and disutility of production.

[5] *Loc. cit.*, p. 104.

be materially greater than as measured by the proposed labour standard.

One objection against the Labour standard recently made by Professor Foxwell does not seem to me decisive: namely that it is impossible to define "a unit of labour."[1] A similar objection might be made to the most generally received index-number based upon consumption; which seems to involve implicitly what Dr. Julius Lehr with his *genusseinheit* has the courage to state explicitly—the measurement of utility.

If the objection is directed, not so much against the difficulty of conceiving, as that of carrying out the labour standard; it may be replied that statistics of wages, which may be regarded as giving the average increase in the amount of money procured by a day's work,[2] are not altogether wanting. For example, Professor Taussig in the paper already referred to exhibits the rise of wages, as well as the fall of prices, during recent years. He remarks:—

"The average, or index-numbers, are in one sense more accurate and significant as to wages than they are as to prices.

"The inevitable fictitious quality of a general index-number thus calls for less constant allowance in using these results of the statistics of wages than in using the figures for prices."

An index-number based on such statistics is accurate enough for the conclusion to which it is applied: *quieta non movere*—that for the purpose of assuring to creditors the produce of a constant quantity of labour an alteration of the standard of deferred payment is not called for.

But this purpose may not be accepted as just and expedient by currency reformers whose end is to minimise the drag on the producer caused by continually shrinking prices.

For the construction of an index-number which should indicate that danger retail prices are less appropriate than wholesale prices. Accordingly when Mr. Cannan, criticising Bimetallism,[3] doubts the fact of appreciation as not evidenced by retail prices he is not persuasive. But the same consideration, with reference to the purpose of endowment—keeping a teacher or

[1] In speaking before Section F of the British Association 1893; as reported in the December number of the *Journal of the Statistical Society*, p. 645. *Cp.* Report of Annual Meeting of the Bi-metallic League, 1894, p. 56.

[2] The other element of effort-and-sacrifice, abstinence, is less easily taken account of. On an average, statistics relating to numerous different occupations, the errors due to the neglect of this element might disappear through compensation.

[3] *Economic Review* October 1893.

preacher on the same level of comfort and respectability—would be pertinent.

It is with the index-numbers as with conduct; in order to form a just judgment, we must always look to the underlying idea and purpose.

As another example of misunderstanding occasioned by diversities of purpose, I may refer to that variety of index-number which purports to determine a real quantity, a cause or characteristic, such as "scarcity of gold," in some more objective sense than a mere fall of prices on an average.[1] The *quæsitum* in this case may be likened to a physical quantity which is to be ascertained from a set of measurements. The method accordingly presents certain peculiarities derived from the theory of errors-of-observation.

I have been unfortunate in not making this view clear to Professor Laughlin. Some years ago[2] he had seemed to deny that there had occurred a general fall in prices in a sense which could prove the existence of "scarcity of gold." After accounting for the fall of prices in several species of commodity, he goes on:—

"The preceding discussion however does not account for a general fall in prices. If the fall of prices had been general, it might suggest a single cause affecting all commodities, such as the scarcity of the medium, by which goods are exchanged, in fact, it seems to be quite necessary to a theory which explains the fall in prices by the scarcity of gold that the fall should be universal."[3]

Referring to this passage and the similar views of other writers I maintained:—

"To assert with Mr. Laughlin and others that, in order to prove a general fall, you must prove a fall in every article, is wholly to ignore the character of the investigation. . . .

"The phenomenon under examination is of the nature of what Mill called a 'residual phenomenon,' like the difference in the mean height of the barometer between two hours of the day, the so-called 'diurnal variation.' On an average of many days there is found to be a fall, but it is not necessary nor true that every day's experience should present that phenomenon. The theory of probabilities is satisfied with a 'majority of days.' . . .

[1] See first Memorandum, Section IX; third Memorandum, Section VI.

[2] In his paper on "Gold and Prices since 1873," in the *Quarterly Journal of Economics* for April 1887.

[3] *Loc. cit.* p. 340.

"It seems to be taken for granted that, when we can show a reason why each price should have varied in the direction actually observed, we are thereby debarred from inferring a general displacement due, in the phrase of Mill, to 'causes that operate on all goods whatever.' But this assumption is quite erroneous. The meteorologist may be able to assign the reason why between morning and noon each particular day there has been a rise or fall of the barometer. But the mathematician is not thereby precluded from extricating by the theory of probabilities a mean variation between those hours." [1]

Referring last year to this criticism Professor Laughlin complains that I have "wholly misunderstood" his argument.[2]

I am very sorry to have unconsciously misrepresented the argument which I was disputing. I can only console myself by reflecting that no amount of care on my part could have averted the mistake, since even after Professor Laughlin's explanation I am unable to discern any appreciable difference between the position which he takes up and that which was the object of my attack. He explains :—

"I at least never contended that 'in order to prove a general fall you must prove a fall in every article.' Accepting the fact of a decline in prices, my contention was solely that the *cause* of the decline could not be scarcity of gold; since, if there was a single cause for the fall then this cause should show itself in all,[3] or nearly all, the commodities quoted." [4]

Now my contention was and is that, though there be a common cause it need *not* "show itself in all or nearly all the commodities quoted."

To take a new example, for which the data happen to be ready to hand : suppose that the average height of a regiment of 1000 Italian recruits selected indiscriminately from all the provinces was returned as half an inch in excess of the average height of the whole army; one might infer with certainty that the difference was due to a real cause (as distinguished from chance); and that cause might well be "single," such as the circumstance that the men in the regiment were (contrary to the general practice) measured with their shoes on. But it does not follow

[1] *Quarterly Journal*, Vol. III (1889) p. 107.

[2] *Journal of Political Economy* (Chicago), Vol. II., p. 279.

[3] His former words (above quoted) are "it is quite necessary . . . that the fall should be universal"; excusing, I think, my expression "a fall in every article." But I am quite willing to substitute "all or nearly all" for "every."

[4] *Loc. cit.*

that this cause should show itself—by excess above the average of the kingdom—in a large majority. The proportion of men above the general average might be about 57 per cent., 570 out of the 1000.* Is that "all, or nearly all"?

* Say the standard deviation for the national stature is 2·6 inches (Yule, *Theory of Statistics*). The coefficient for the mean height of the regiment would be ·08; less than a *sixth* part of the observed difference! If $h$ is the average height of the army, $h + {\cdot}5$ that of the regiment, the proportion of the regiment above $h$ would be about 57 per cent. (2·6 being the S.D.).

## (K)

## PIERSON ON SCARCITY OF GOLD

[THIS is the first of two articles in which I deal with writings of the great Pierson relative to index-numbers. In this article, published in the ECONOMIC JOURNAL, March 1895, I have nothing but praise for Mr. Pierson's treatment of the subject.]

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The Bimetallic League ought to translate and circulate an article on *Scarcity of Gold* which has lately been contributed by the eminent ex-professor, and ex-minister, Mr. N. G. Pierson, to the Dutch periodical *De Gids*. Lessons of caution and moderation might be accepted from such a teacher. For Mr. Pierson owns to a certain sympathy with the Bimetallist party. Indeed, he has ranged himself on that side unequivocally in his well-reasoned communication to the Gold and Silver Commission (second Report), in the course of which he says:—

"It is considered a mere truism in this country [Holland] to say that Bimetallism, though highly objectionable if applied in a small country, is the best system imaginable if applied by an international agreement in a large number of civilised States. We thoroughly believe that it would be a great boon to all nations if this system were adopted by the principal countries of Europe and America."

Mr. Pierson in the article before us proposes two questions: I. Has there been a rise in the purchasing power of gold? II. If so, is the cause connected with gold or goods, or both?

I. The first question involves a consideration of the method of index-numbers. Mr. Pierson prefers the arithmetic to the geometric mean; not without a certain deference to the "feeling"—rather than the arguments—of Jevons, in favour of the geometric mean. Distinguishing the simple average of relative prices from that which is weighted according to the *importance* with respect to some human interest of different commodities, Mr. Pierson very properly ascribes a certain validity to the simpler

and more objective mean, for its own sake, and apart from the circumstance that as a matter of fact the two procedures are likely to differ little in result.

As to the period which should be taken as the base of standard, it is a matter of complaint that the advocates of Bimetallism frequently select the "inflation period" of 1870—75. If the period 1861—70 be taken as base, the period 1881—83 compared therewith shows no fall of prices; according to the index-numbers of Soetbeer and Dr. Kral, based respectively on 114 and 265 commodities. It is true that Mr. Sauerbeck's index-numbers do point to a fall of prices in the interval considered. But this discrepancy is due to Mr. Sauerbeck's not having used a sufficient number of data. For observing that thirty-five of Mr. Sauerbeck's articles—or rather forty-one, as six of Mr. Sauerbeck's articles are duplicated—are common to Soetbeer, let us substitute the prices of those articles used by Sauerbeck for the prices used by Soetbeer in his index-number; and that index-number for the period 1881—83 compared with the period 1861—70 will not be appreciably affected. The discrepancy between the two results is found to be mainly due to the seventy-nine articles which Soetbeer has, and Mr. Sauerbeck has not. Accordingly it would seem that Soetbeer's result is the better founded. Comparing, according to his method, the level of prices in the period 1885—91 with that of 1861—70, we find a rise in the purchasing power of gold of only some 16 per cent.; whereas it is usual on Bimetallist platforms to speak of a greater rise.

II. Coming to his second question, Mr. Pierson claims against both parties the right to use the term *appreciation* in the sense of a rise in the purchasing power of gold due to causes affecting gold primarily. He seems to convict a leading monometallist organ of using the term inconsistently. His difference with the Bimetallist leader, Professor Foxwell, is rather about things than words. Mr. Pierson controverts the argument that the depression which has prevailed during recent years must have been due to monetary disturbances: for that there are no other adequate causes for it. There have been many other causes, replies Mr. Pierson, and the cause assigned is not adequate.

To take the latter point first: the Bimetallist in his gloomy picture of the evils of contraction is apt to leave out of account the classes who are benefited by a fall of prices. These are not only creditors not engaged in active industry, but also certain classes of producers. Consider the series of instruments and materials conducive to the production of goods ready for consump-

tion—the goods of the second or higher orders in the phraseology of the Austrian economists. Suppose that a fall of prices occurs first in the goods which are of the highest order, and is propagated downwards. Each class of producers, while his expenses of production are diminished, and until the price of his finished product falls, pending the restoration of economic equilibrium, is benefited. Who shall say that the fall of prices is not as likely to move in the direction which has been described as in the opposite direction? Thus Mr. Pierson is unable to accept Mr. H. H. Gibbs's dictum—that contraction is a greater evil than inflation. In this connection and with respect to other assertions of unfashionable opinions, Mr. Pierson refers with approbation to Professor Marshall's masterly and impartial evidence before the Royal Commission on the relative values of the precious metals.

The allegation that monetary disturbance is the only adequate cause of the recent depression of trade Mr. Pierson meets by assigning other causes: the very dislocation caused by improvements in production, the Protection which became rampant in the 'seventies, the fall of prices consequent upon recent improvements in transportation and upon the enormous increase of goods which has occurred in so many departments of production—augmentations which Mr. Pierson, following in the steps of Mr. David Wells, exhibits in imposing detail. It is true that some of these causes, especially the last, operated at earlier periods, but not, Mr. Pierson seems to think, in the same degree. Perhaps he has hardly considered the difficulty of proving such a difference in degree.

Mr. Pierson however concludes with confidence that the level of prices did not fall till after 1883, and that the fall is due to causes connected with goods, not gold. But while thus cutting away one of the principal planks of the Bimetallist platform, Mr. Pierson does not attach himself to the opposite party. He makes a distinction between the creed and the propaganda of the Bimetallists. He believes in an international arrangement for steadying the relative value of gold and silver. But this arrangement need not involve the principle of unlimited coinage at a fixed ratio.

One practical difficulty in the application of that principle may be stated in the form of a dilemma as follows. If the ratio adopted is considerably different from $15\frac{1}{2}$ (silver) : 1 (gold), say 25 to 1, then the expense of introducing the change would be enormous in view of the depreciation of the existing stocks of silver current at the rate of $15\frac{1}{2}$ : 1. "If I reckon rightly," says

Mr. Pierson, "it would cost Holland, exclusive of her colonies, about 52 million florins; France about 950 million francs." But if the $15\frac{1}{2}$ is to be adopted, then there is likely to be caused an immense appreciation of money throughout the East—the last result that a consistent Bimetallist can approve.

Mr. Pierson's own plan for keeping the relative value of gold and silver constant is a modification of the general idea that the central banks in Europe—in the United States the Treasury—should be required to purchase the metals at a fixed price. But perhaps it is unnecessary to go into particulars, as Mr. Pierson admits that there are three fatal objections to the practical adoption of his plan.

While pointing out the difficulties of remedial action, Mr. Pierson does not, like the monometallists, deny the existence of monetary disease. It is a sad conclusion that things are in a bad way. It is a poor consolation that they might have been worse. For instance, the success of the monetary experiment in the Dutch colonies is much more perfect than could with reason have been expected.

These conclusions are corroborated by a communication which Mr. Pierson has made to the January number of the Dutch monthly *De Economist*. In this number (p. 64) Mr. Pierson discusses the statistics of prices which Mr. Heinz, the chief of the Hamburg Statistical Bureau, has prepared in continuation of the work of Soetbeer [compare the statistics referred to in the ECONOMIC JOURNAL, Vol. IV. p. 201]. Soetbeer's series of index-numbers was interrupted by tariff-regulations which disturbed the prices of several commodities in such wise as to render them after 1891 no longer commensurate with the prices of the same articles for earlier years. Accordingly Mr. Heinz has to confine himself to articles which have been imported by sea into Hamburg. Operating with 137 articles of this class, Mr. Pierson constructs a series of index-numbers which is continuous from the year 1850 to the date of the most recent returns. Unfortunately for the comparison with Soetbeer's figures, Mr. Pierson has been compelled, by the imperfection of his materials, to take as the base of the new index-numbers the year 1850, instead of the period 1847–50 which Soetbeer had taken. But in spite of this discrepancy, and the more serious difference in the mode of construction, the parallelism between the two sets of index-numbers is wonderfully close, as the annexed figures show :—

| | Heinz. | Soetbeer. |
|---|---|---|
| 1850 or 1847–50 [1] | 100 | 100 |
| 1851–55 | 110·80 | 112·22 |
| 1856–60 | 119·88 | 120·91 |
| 1861–65 | 120·23 | 123·59 |
| 1866–70 | 118·44 | 123·57 |
| 1871–75 | 131·57 | 133·29 |
| 1876–80 | 120·88 | 123·07 |
| 1881–85 | 114·73 | 117·68 |
| 1886–90 | 105·53 | 104·40 |
| 1891 | 111·55 | 109·19 |

[1] 1850 for Heinz's, 1847–50 for Soetbeer's index-number.

It will be observed that the new series like the old one, when the "inflation period" of 1871–75 is left out of account, shows no signs of appreciation due to monetary disturbances of that period: thus confirming Mr. Pierson's view that the appreciation which we now experience is due to causes connected with goods rather than gold.

The congruity between the two Hamburg index-numbers is such that they mutually support each other. On the one hand additional strength is imparted to the conclusion which Mr. Pierson had obtained on independent grounds (above p. 352), that Soetbeer's index-numbers are more trustworthy than Mr. Sauerbeck's. On the other hand the index-numbers constructed by Messrs. Heinz and Pierson, having agreed closely with Soetbeer's for forty years from 1850 to 1891, may be presumed to be almost as trustworthy as Soetbeer's index-numbers would have been for the years after 1891.

If we compare the period 1886–93 with the period 1861–70 taken for base—as recommended by Mr. Pierson in his study on the scarcity of gold (above p. 352)—we shall find from Mr. Heinz's materials, the index-number 88·8; from Mr. Sauerbeck's, 69·5. According to Mr. Pierson the former measure of appreciation is the more trustworthy; but the latter is more convenient for the advocates of Bimetallism.

(L)

## A DEFENCE OF INDEX-NUMBERS

[In this article, which appeared in the Economic Journal, March 1896, I ventured to differ from Mr. Pierson's "Further Considerations on Index-Numbers" (published in the same number of the Journal), on the ground that they ignore the character of Probabilities essential to the computation of index-numbers.]

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It is justly observed by Adam Smith that the anxiety about public opinion is much greater among the candidates for excellence in some arts than it is in others. "The beauty of poetry is a matter of such nicety that a young beginner can scarce ever be certain that he has attained it. . . . Racine was so disgusted by the indifferent success of his *Phèdre*, that though in the vigour of his life and at the height of his abilities, he resolved to write no more for the stage. . . . Mathematicians, on the contrary, who may have the most perfect assurance both of the truth and of the importance of their discoveries, are frequently very indifferent about the reception which they may meet with from the public." [1] In the scale of susceptibility which is thus indicated, a high place must be assigned to the more refined parts of economic science. Even those investigations which at first sight appear to be wholly statistical—such as the calculation of index-numbers—may rest upon speculative assumptions, concerning which the consensus of authority is naturally desired. Accordingly, when the distinguished Dutch economist concludes in the immediately preceding paper that "all attempts to calculate and represent average movements of prices, either by index-numbers or otherwise, ought to be abandoned," those who have been making such attempts will anxiously reconsider the basis of their computation, and tremble for its safety. But the discouragement which such a condemnation coming from such an authority is calculated to produce may be mitigated by observing that the index-number which is the object of Mr. Pierson's crushing criticisms is one of a very peculiar character, differing in some essential attributes

[1] *Theory of Moral Sentiments*, Part III. ch. ii.

from the operation as ordinarily conceived and practised. Racine would not have been dejected by the indifferent success of his tragedy if the play, so badly received, had been a version of his masterpiece from which the characters of Phèdre and Hippolyte had been left out. Two equally serious omissions are presupposed by Mr. Pierson's animadversions.

There is, *first*, the character of probability. It is generally implied that the problem now before us, in its data, method, and result, is germane to the Calculus of Probabilities. The nature of the problem is happily indicated by Professor Nicholson when he compares the set of moving prices to a fleet of yachts which under the influence of a common cause—it may be rising wind or tide—are variously accelerated according to "the build of the various yachts or seamanship of the crews." The type of such problems is the investigation of what Mill calls a *residual phenomenon*,[1] illustrated by the discovery of the diurnal variation in the height of the barometer by comparing the averages of a great number of observations at different times of day. It is postulated in such reasoning that the error or deviation of one observation is independent of that which has been incurred by another observation;[2] just as, when a die is thrown a number of times, it may be assumed that the number of pips turned up at each throw is unaffected by the preceding throws. It is true that in concrete nature such ideal independence can hardly be expected. Thus in barometrical observations it is possibly not correct to treat the observation for each day as an independent sample. Probably the weather sometimes follows suit for two or three days together; but the deviation of the observations is doubtless sufficiently random to justify Laplace's application of the Calculus of Probabilities. So the grouping of human statures is perhaps not perfectly sporadic;[3] but it is sufficiently so to allow a Galton to infer with great probability that the conditions of a particular class—*e.g.*, boys in public schools, or men in the Royal Society—as compared with less favoured classes are particularly favourable to growth.[4] It is not necessary to

[1] Mill, *Logic*, Book III. ch. xvii. *Cp.* Laplace, *Probabilities*, Book II. ch. v.

[2] On this postulate see the present writer's "New Methods of Measuring Variations in General Prices," *Journal of the Royal Statistical Society*, 1888, p. 367, note. *Cp.* Laplace, *loc. cit.*: "Il faut avoir soin de varier les circonstances de chaque observation."

[3] As appears from the fact that in the group constituted by the measurements of a nation there will be sub-classes with different averages.

[4] *Cp.* "Methods of Statistics," *Journal of the Royal Statistical Society*, Jubilee volume.

discuss here whether the average would be of any scientific use if this condition of sporadic dispersion were not fulfilled—if all the observations were massed at two points, or collected into two sharply demarcated classes—*e.g.*, dwarfs and giants.[1] It is sufficient to observe that as a matter of fact the condition of sporadicity is very generally fulfilled both in physics and social phenomena: wherever there is at work a set of miscellaneous agencies, "a mass of fleeting causes" in Mill's phrase.[2]

It is by ignoring this character of sporadic dispersion that Mr. Pierson's criticisms acquire their plausibility. He begins: "Let us suppose ten commodities, all equally important. Five of them are doubled in price, and five of them fall to exactly one-half." But surely this is a very odd supposition, in view of the sporadic dispersion which very generally prevails in this world. It would have been more appropriate to suppose a number of figures representing variations of price (in one epoch as compared with another), not separately disposed in two heaps, but scattered about. Mr. Pierson's supposition would be appropriate if, for instance, Mr. Sauerbeck's percentages for the comparative prices of different commodities were massed at two points. But this is not so, as appears by considering his figures and diagrams representing annual or quarterly variations of price.[3] A common trend comes out in the average, but the particular movements are independent.

The recognition of this sporadic character is fatal to Mr. Pierson's principal objection, which is in effect, though perhaps not apparently, that if the particular observations be weighted differently the average will be seriously different. This objection recurs in different forms. In his first paragraph Mr. Pierson supposes ten observations: five commodities of which the price has been doubled, five of which it has been halved; in the second as compared with the first period the data may accordingly be regarded as consisting of ten ratios, or percentages, five of them each = 200 (: 100); five of them each = 50 (: 100). The simple arithmetical average of these may be written

[1] See the reference given in the preceding note. See also p. 279 in the Memorandum on *Methods of Ascertaining and Measuring Changes in the Value of the Monetary Standard*, by the present writer, published in the Report of the British Association for 1887. This Memorandum and the two supplementary ones, published in the Reports of the British Association for 1888 and 1889, should be referred to as containing justifications of statements made summarily in the present paper.

[2] Mill, *Logic*, loc. cit.

[3] A similar scrutiny of Laspeyres' statistics of relative prices is attempted in the Memorandum of 1887 [H, above, p. 245.]

$\dfrac{5 \times 200 + 5 \times 50}{10} = 125$. Now *weight* [1] each observed percentage with its own reciprocal, and you have $\dfrac{5 + 5}{5 \times \cdot 005 + 5 \times \cdot 02} = 100 \div 1{\cdot}25$. And the complaint is that these two results are not equal.

The complaint is virtually similar in the sixth paragraph (*loc. cit.* p. 128). There the simple observations are 75, 16·66, 25. And the simple arithmetical mean is $\dfrac{75 + 16{\cdot}66 + 25}{3} = 38{\cdot}88$. The other average which is contrasted with this one is obtained by weighting each observation with the value in money of a pound avoirdupois of the corresponding commodity at the initial period, that is 20, 12, 4, respectively. These weights being applied, the average becomes $\dfrac{1500 + 200 + 100}{60 + 12 + 36} = 16{\cdot}\dot{6}$.

The same contrast is noticed in some other cases. "In Case I. there will be no change," "in Case II. there will be a rise of 25 per cent.," "in Case III. there will be a fall of 25 per cent."; the observations being weighted in the peculiar mode [2] which has just been described; whereas, according to the simple arithmetic mean, there is no change in any of the cases.

Such discrepancies seem very serious when we deal with artificially simplified examples; but they become insignificant when we deal with the concrete, sporadically dispersed, price-ratios. For it is a well-known proposition that a difference in the system of weights will not make much difference, provided that the number of independent observations is sufficiently great; provided also that the experiment is made in the spirit of Probabilities, with an *animus mensurandi*—in Herschel's phrase—not consciously selecting cases which will not work well. The reason and limits of the proposition are defined by theory,[3] and the theory is confirmed by experience.

As verifications of the theory *in aliâ materiâ* may be adduced the index-number constructed by Mr. Bowley to indicate the

[1] If $x_1, x_2, \ldots x_n$ are observations, the simple arithmetic mean is $\dfrac{x_1 + x_2 + \ldots + x_n}{n}$ the weighted arithmetic mean is $\dfrac{w_1 \times x_1 + w_2 \times x_2 + \ldots + w_n \times x}{w_1 + w_2 + \ldots + w_n}$, where $w_1, w_2, \ldots, w_n$ are the *weights*.

[2] The peculiarity of the mode being to assign as weight a pound or bushel, or, as in the passage before us, some unit, which is arbitrary and accidental with reference to the measurement of the depreciation of money. See below, p. 367.

[3] See I, above, p. 305 *et seq.*

increase of general wages. Weighting the percentages expressing the growth of wages in America according to the system which he thinks best, and according to the very different system employed by the American statisticians, Mr. Bowley obtains almost exactly the same result.[1] Another conspicuous example is afforded by the concurrence between the different methods which Sir R. Giffen in his census of wages has employed in order to determine the average wage. Using, in effect, different systems of weights, he obtains for the average weekly wage the values 29*s*. 5*d*., 29*s*. 7*d*., 29*s*. 7*d*.[2]

Experience more adjacent to the case in hand is afforded by the price-ratios which Mr. Sauerbeck has tabulated year by year. There is found to be a close agreement between the arithmetic mean and the averages which are obtained by taking account of quantity. The following figures are given by Mr. Sauerbeck in the ECONOMIC JOURNAL for June 1895 :—

SIMPLE AND WEIGHTED AVERAGE OF COMPARATIVE PRICES.

| — | Arithmetical mean. | Making allowance for quantities. |
|---|---|---|
| 1887 | 68 | 66·7 |
| 1888 | 70 | 68·8 |
| 1889 | 72 | 71·8 |
| 1890 | 72 | 72·1 |
| 1891 | 72 | 72·0 |
| 1892 | 68 | 67·7 |
| 1893 | 68 | 67·1 |
| 1894 | 63 | 62·0 |

Other comparisons of the two kinds of average are given by Mr. Sauerbeck in his well-known papers in the *Journal of the Statistical Society*. Further verifications will be found in the second of the Memoranda above referred to. It will be sufficient to make one extract. Of the percentages indicating the variations in price of nineteen commodities tabulated by Mr. Palgrave, the simple arithmetic mean and the mean weighted according to quantity are compared for sixteen successive years, and the sixteen differences between the two results for each year are as follows : [3] 4, 2, 2, 3·5, 1, 5, 0, 1, 2, 0, 4·5, 7, 2, 2·5, 1, 1.

[1] ECONOMIC JOURNAL, Vol V. p. 373.

[2] *Report on the Wages of the Manual Labour Classes.* [C. 6889—1893.]

[3] H, above. See also pp. 202 and 205. Attention may be called to the experiments with weights assigned arbitrarily : by forming the sum of a set of digits taken at random (p. 199, last paragraph), or—in the cognate case of the Median—tossing up a coin and assigning 1 or 2 as the weight, according as head or tail turned up.

But it is needless to labour this proposition further, as it is acknowledged by Mr. Pierson in a former paper[1] when he deals with real examples: in particular Mr. Palgrave's index-number, and Mr. Falkner's report on "wholesale prices,"[2] in which the simple arithmetic mean of some hundreds of relative prices and the mean of the same weighted according to the importance of each commodity in the average household budget are found to agree. Here are some of the figures quoted by Mr. Pierson:—

| — | Ordinary Average. | Corrected [weighted] Average. |
|---|---|---|
| 1871–75 | 134·58 | 131·26 |
| 1876–80 | 106·78 | 108·14 |
| 1881–85 | 102·52 | 104·0 |
| 1886–90 | 93·04 | 95·20 |

"It is clear," comments Mr. Pierson, "that the relative weight may be left out of consideration without marked detriment when we extend our investigation to a great number of articles."

To sum up, several of Mr. Pierson's objections amount to this one: that the calculation of average relative prices is untrustworthy, because the result is seriously different according as different systems of weighting are employed. And this objection, though true in the abstract of artificially simplified index-numbers, is not true of the sets of figures with which we have actually to deal.

A similar reply may be made to the objection that the result of the calculation will be seriously different according as the arithmetic or the geometric mean is employed. This is true of the imaginary examples set up to be knocked down, but it is not true in the concrete. The arithmetic and geometric mean of the price-ratios for a large number of miscellaneous commodities are likely not to differ much from each other. This is a deduction from a more general proposition that, with certain reservations, *any* mean of a large group of observations is likely not to differ much from any other kind of mean.[3] Take, for example, the series of observations obtained by measuring the heights of different men. The arithmetic mean[4] of 1000 such observations obtained

[1] The paper described in the ECONOMIC JOURNAL, Vol. V. p. 109. See p. 8 of the German edition of *Goldmangel* (reprinted from the *Zeitschrift für Volkswirthschaft*, Band iv. Heft 1).

[2] Well summarised by Professor Taussig in the *Yale Review* for November 1893.

[3] For the evidence and limits of this proposition see the paper on the "Law of Error," by the present writer, in the *Philosophical Magazine* for November 1892. It is supposed that, as usual where miscellaneous agencies are at work, the *law of error* is approximately fulfilled by the observations; also that these are measured from a point outside the extreme value which an observation can possibly reach: for example, in the case of human statures or price-ratios, *zero*.

[4] The observations are given in the paper just referred to. Each of them is the mean height of twenty-five men.

by Mr. Elliott is 68·20 inches. Compare with this the mean value which is obtained by squaring all the observations, taking the arithmetic mean of the squares, and extracting the square root of that mean. The mean value so obtained is 68·25. The mean value obtained by cubing all the observations, taking the arithmetic mean of the cubes, and extracting the cube-root of that mean, is much the same, viz. 68·30. The geometric mean is 68·16.[1]

To adduce more specific experience, here are two rows of figures, of which one consists of the geometric means of thirty-nine percentages obtained by Jevons for several years, the other consists of the arithmetic means of the same percentages.[2]

| | 1851. | 1853. | 1855. | 1857. | 1859. |
|---|---|---|---|---|---|
| Geometric Mean .......... | 92·4 | 111·3 | 117·6 | 128·8 | 116 |
| Arithmetic Mean ......... | 94·6 | 112·4 | 119 | 134 | 119 |

Mr. Sauerbeck has calculated the geometric mean of his forty-five percentages for two years and allows me to cite the results [3] :—

| | 1880. | 1894. |
|---|---|---|
| Arithmetic Mean [4] .............. | 87·82 | 62·93 |
| Geometric Mean .................. | 86·97 | 60·90 |

So much for the objection implied in the preceding paper that

[1] These calculations have been performed by Mrs. Bryant, D.Sc.

[2] From the Memorandum of 1888, p. 206. Alternate years were taken, the more to vary the circumstances of the experiments. There is no reason to suspect that successive years would have presented different results. For instance, for 1852 the geometric mean is 93·8, the arithmetic 94·6 (*loc. cit.*).

[3] That is, the simple arithmetic mean. The weighted (arithmetic) means were respectively 87·3 and 62·0.

[4] The geometric mean comes out a little less than the arithmetic, as might have been expected. This tendency may confer some advantage, but a very slight one (Memorandum, 1887, pp. 283–289), on the geometric mean. A more important prerogative of the geometric mean was noticed, as far as I know, first by Professor Harald Westergaard, and has not been sufficiently recognised by the *connoisseurs* of index-numbers. The geometric mean is the only one in which no alteration at all is produced by the change of basis. In the case of the arithmetic mean, if one year, $x$, be taken as basis, and the index-numbers for $y$ and $z$, say $I_y$ and $I_z$, be determined as percentages with reference to $x$, then the ratio of $I_y$ to $I_z$ will not in general be exactly the same when the index-numbers are calculated with reference to another basis, $x'$, say $I'_y$ and $I'_z$. The reason is that (as explained above with reference to a particular case where $x = y$ and $x' = z$) the two ratios $\frac{I_y}{I_z}$ and $\frac{I'_y}{I'_z}$ are to be regarded as *differently weighted* means of the same set of observations, viz. the set of ratios obtained by dividing the price of each commodity in $y$ by its price in $z$. That the geometric mean follows in this respect the analogy of physical measurements is at least an elegance. The geometric mean is *pro tanto*—I do not say more accurate, but—more plausible than others. Unlike the arithmetic mean, it is not at all affected by the paradox pointed out by Mr. Sauerbeck in his article in the ECONOMIC JOURNAL (Vol. V. p. 163), that the extent of a fall (or rise) appears slightly different according as we start from a high or low basis

the index-number is the sport of the particular system of weights or species of mean which may be adopted. It is a more serious objection, expressed in former papers, that the result is materially affected when we take in additional data,[1] combining with Mr. Sauerbeck's forty-five prices the sixty-nine other prices treated by Soetbeer or his successors.[2] To reply that these commodities are unimportant in respect of quantity does not appear to me permissible *so long as* we treat the problem as simply statistical and purely objective.[3] From this point of view the *quæsitum* is such as the average barometric pressure at a certain time of day, to be ascertained, it might be, from observations with different barometers. For this scientific purpose there would be no propriety in attaching more importance to the observations made with barometers in which the column of liquid had a larger sectional area.[4]

The case may be as if it were required to find the average rise of the tide along an indented shore by observing the height of the water in several creeks. If the average of forty-five observations was materially altered by taking in sixty-nine additional ones we might conclude that we had not at first observed a sufficient number of samples. Perhaps we should have to content ourselves with a very rough figure, unless we took into account some practical purpose for the sake of which the measurement was undertaken. For instance, with reference to the purpose of using the reflux of the tide for the generation of energy, it might be desired to have a measure of the comparative number of foot-pounds available at different seasons. With reference to such a purpose no great error would be incurred by leaving out of account the smaller creeks. In such a case the Calculus of Probabilities by itself could tell us only the whereabouts of the

[1] This transition corresponds to division (3) of the analysis in the second Memorandum, p. 190 *et seq.* As observed there (p. 194), there is a greater inductive hazard involved in passing to new commodities than in allowing for inaccuracy in the weights of a constant set of commodities.

[2] See ECONOMIC JOURNAL, Vol. V. p. 110, and Mr. Sauerbeck's article in the same volume.

[3] As I understand Mr. Pierson to mean in the first paragraphs of the extract from his *Goldmangel* given in his article in the ECONOMIC JOURNAL (Vol. V. p. 331).

[4] Unless, indeed, there were some ground for believing that the smaller size was accompanied with some defect in the qualities of a good measurer: that the observations afforded by the thinner tube, or the commodity consumed in smaller quantities, were more liable to disturbance, or less independent, than other observations. Mr. Sauerbeck has suggested some reason for believing this in the case of commodities which are commercially unimportant (ECONOMIC JOURNAL, Vol. V. p. 171). Another reason has been suggested by the present writer (H, above, p. 247).

required average; the estimation of utility must be called in to render the result precise.

The direction to a practical purpose is the *second* attribute of an index-number which Mr. Pierson leaves out of account—not, indeed, ignoring this property, but deliberately omitting it, for reasons which he has given in a former paper.[1]

"One person consumes much bread and little meat; one person smokes tobacco, another drinks wine, a third neither smokes nor drinks, but makes a collection of books and etchings. In order to judge of the influence on the material condition of men exercised by the variations of prices it would be necessary to divide people into numerous groups, because the relative importance of commodities differs according to individual wants."[2]

There is, no doubt, much wisdom in these reflections; and I fully admit that the eminent author in his earlier and more temperate criticism of index-numbers has made important contributions to the determination of the probabilities and utilities that are pertinent to the subject. I submit, however, the following considerations as a counterpoise to his present scepticism:—

(1) Is it certain that the ground of weighting the variations in price according to their importance with reference to human welfare must be of the subjective kind just considered: taking account of individual wants? Is not a more objective criterion afforded by the increase in the amount of currency which would be required, in the case of appreciation, to raise a commodity to its original price, according to which criterion more weight should be assigned to those commodities which, being circulated in greater quantities, make greater demand on the currency?[3]

(2) With respect to more subjective determinations of importance, the mere diversity of tastes would not be fatal, I think, provided that the expenditure of different individuals is distributed among the different individuals in a normally sporadic fashion,[4] so that a particular system of quantities of commodities consumed tends to occur with maximum frequency, other systems

[1] Economic Journal, Vol. V. p. 331, quoting from *Goldmangel*, pp. 8–10.

[2] *Cp.* Professor Marshall in the *Contemporary Review* for 1887, p. 372.

[3] The allusion is here to the method described in the third of the Memoranda as Professor Foxwell's method (H, above, p. 261). "In averaging the respective price-variations he would assign to each an importance proportioned to the corresponding value." . . . "The question set to us is a pure currency question; and the answer to be sought primarily is not by how much are debts to be scaled up or down, but by how much the metallic currency is to be multiplied in order that the monetary *status in quo* may be restored."

[4] The variations in the quantities consumed with the price (Pierson, *loc. cit.*) might, I think, be treated as magnitudes of the second order.

with less and less frequency according to a well-known law.[1] It must be presumed also that the income of the individuals is about the same [2]—or, rather, distributed normally about an average. Under these circumstances it would be proper to take the average quantities consumed for the weights of the price ratios.

Where these conditions are not fulfilled the proper course would seem to be to construct index-numbers for the different strata of society each of which may have a *type* of expenditure and income in the sense above indicated. The various index-numbers thus constituted would almost certainly differ from each other less than Mr. Sauerbeck's and Soetbeer's (in recent times) have done; they would probably agree better with Mr. Sauerbeck's index-number, in which the component commodities are selected with some regard to their importance to the consumer, than with Soetbeer's, in which no such selection is made.

If practical exigencies require that some one measure of utility should be framed by combining the index-numbers pertaining to different strata of society, then presumably more importance should be assigned to that one which pertains to the masses.[3]

Upon some such principles may be justified the conclusion which Mr. Sauerbeck reaches in his discussion of this matter in the ECONOMIC JOURNAL, Vol. V. p. 171: "Small articles should not be taken acount of in an index-number constructed like Soetbeer's" (that is, a simple arithmetic average of relative prices).[4]

Let it be freely admitted that this measurement of utility has

[1] The prevalence of the *Compound Law of Error*, or probability function of several variables, is proved for the attributes of organisms by the researches of Messrs. Galton and Weldon. With respect to its prevalence and significance in social phenomena see *Statistical Correlation between Social Phenomena*, by the present writer, in the *Journal of the Statistical Society* for December 1893.

[2] On the conditions postulated for the measurement of utility see Professor Marshall's *Principles of Economics*, 3rd edition, Book I. ch. iv.

[3] Because they are more in number and the final utility of money to them is greater.

[4] Analogous remarks apply to the construction of an index-number for measuring the appreciation or depreciation of money, not by the variation in the utility, which is procured by the unit of money, but by the variation in the disutility of labour, by which a unit of money is procured. This is the Labour Standard discussed in the third Memorandum (1889). This method of measuring appreciation has been adopted by Professor Simon Newcomb and some other eminent writers (see J, above, p. 345). It has been unfavourably criticised by Professor Foxwell in the *National Review* for January 1895. No doubt the measurement of appreciation in terms either of disutility or of utility becomes a delicate matter when the production and the consumption of goods per head vary. The subject has been recently discussed by several able writers in the American Academy for Political Science.

not quite the objective character of physical science. It may nevertheless be a postulate of practical economics.[1]

It sometimes happens that an original thinker who rebels against unscientific assumptions himself assumes first principles which are not more demonstrable than the received ones. Of this character, if my interpretation is right, is Mr. Pierson's tacit assumption that the *primâ facie* proper method of dealing with observed variations in price is, in his own words: "If a pound of sugar, a pound of wheat, a yard of cotton yarn, and whatever else is purchasable could be bought in the period 1847–1850 for a sum of money which we call 100, and this sum of money has risen in the period 1851–1860 to 116, we are fully entitled to conclude that the purchasing power of money in those years has fallen in the proportion of 116 to 100." [2]

And, again: "Let us suppose three commodities, costing (A) 20*d.*, (B) 12*d.*, and (C) 4*d.* a pound, and falling respectively to 15*d.*, 2*d.*, and 1*d.* a pound. This will be an average fall at the rate of 100 to 50, for—

$$20 + 12 + 4 = 36d.$$
$$15 + \ \ 2 + 1 = 18d.$$

In other terms, twice the quantities of these commodities will be purchasable for the same amount of money as before." But "index-numbers" (that is, the ordinary arithmetic mean of the price-ratios expressed as percentages) will show a fall from 100 to 38·88. "Which is manifestly wrong," says Mr. Pierson.

And, again, of the ordinary arithmetic and geometric mean of price-ratios, "both methods are wrong": as disagreeing with a method which in its essential feature resembles that which has just been described. If we consider the ratio between the prices at different epochs to constitute the datum of observation, Mr. Pierson's method of combining these data is to weight each observation with the money-value of the unit of avoirdupois or volume measure.[3]

Where is the peculiar propriety of this system of weighting, according to which a variation in the price of, say, *argon* or *iridium* should count for more than a variation in the price of *coals* or *cotton*, because each pound-weight of the former articles is dearer than a pound-weight of the latter? I do not now so much complain that the system has no reference to any useful purpose.

[1] The practical validity of index-numbers is well shown in Mr. L. L. Price's excellent *Money and its Relation to Prices*.

[2] Economic Journal, Vol. V. p. 331.

[3] Cp. H, above, p. 258.

The statistician is within his rights in making abstraction of human welfare; but, viewing the problem as purely objective and merely statistical, why should we employ this principle of preference? [1]

Let us, after Mill [2] and Hume, represent the phenomenon under consideration, depreciation,[3] by supposing that, *ceteris paribus*, every piece of money and instrument of credit has been on an average increased in a certain ratio. With reference to the measurement of that ratio [4] it is surely an accidental circumstance whether the unit of mass or volume of one commodity as compared with another exchanges (prior to the depreciation) for more or fewer units of money. The following would be a fair analogy in physical measurement of the proposed system of weighting. Let it be required to determine the expansion due to a rise in temperature for the diamond, from observations made on several portions of the substance. Lay out several units of money—say pounds sterling or ten-pound notes—in purchasing so many parcels of diamonds. Make an observation with each of these portions, and *weight* each observation with the mass or the volume of the diamond which is obtained in exchange for the unit of money. According to this arrangement an observation on a compact and glittering diamond shall count for less than one made upon a mass of less commercial value. This system of weighting the observations is on a par with Mr. Pierson's system. The number of units of mass or volume exchanged for the unit of money is not more irrelevant in the physical measurement than is the number of units of money exchanged for the unit of mass or volume in the monetary measurement.

Yet Mr. Pierson treats this system as *primâ facie* reasonable, and abandons it only because its two modes—mass and volume—lead, in imaginary examples, to inconsistent results. And he deduces from this inconsistency the futility of the whole measurement: that "all attempts to calculate and represent average movements of prices either by index-numbers or otherwise ought to be abandoned."

Let us see how this sort of objection would apply to the typical physical problem above instanced, the determination of the diurnal

[1] The principle does not seem to have found much favour among the constructors of index-numbers. It is mentioned by Geyer in his *Theorie und Praxis der Zettelbankkursens*, appendix vi. But he at once introduces a modification which makes his system practically identical with the ordinary arithmetic mean.

[2] Mill, *Political Economy*, Book III. ch. viii. § 2.

[3] Or, *mutatis mutandis*, appreciation.

[4] Another appropriate conception of the *quæsitum* might be, I think, the change in the quantity of final utility which is the equivalent of the unit of money, assuming the marginal utility of goods not to have altered.

variation of the average atmospheric pressure. Suppose that the observations have been made with barometers consisting of different liquids—mercury, water, etc. Weight each observation *first* inversely as the money-value of the unit-weight of the corresponding liquid, and *secondly* inversely as the money-value of the unit-volume of the liquid. Then, *if* the observations are not sporadically dispersed, but collected at two or three points, it will make all the difference whether the first or the second system of weighting be employed. Therefore the calculation of average variations in barometric pressure—performed by Laplace and approved by Mill—is to be "abandoned altogether" as "faulty in principle."

## (M)

# MR. WALSH ON THE MEASUREMENT OF EXCHANGE-VALUE [1]

[THE ground on which I ventured to criticise Mr. Pierson's attack on index-numbers, namely, the not to be ignored connection of the subject with Probabilities, is also the main ground of my differences with Mr. Correa Walsh. They are expressed in the following paper, which appeared in the ECONOMIC JOURNAL, 1901, under the title, "Mr. Walsh on the Measurement of General Exchange-Value." Mr. Walsh does not accept my view, and has replied with vigour in a brochure entitled "The Problem of Estimation," of which an account is given in the *Journal of the Statistical Society* for 1921, in a review bearing the well-known initials G. U. Y.

A rejoinder to Mr. Walsh's replies is published in two parts, one in the ECONOMIC JOURNAL, September 1923, the other in the *Journal of the Royal Statistical Society*, July 1923. *Cp.* above, p. 198.]

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The capacity of taking boundless trouble, which is a characteristic of solid talent, distinguishes the work of Mr. Walsh. Whether he searches the writings of others or elaborates his original ideas, the thorough student and close thinker is manifest on every page.

The literature of the subject has never been examined so fully. Every devious path in the field where index-numbers flourish has been traversed in order to form an unrivalled collection of methods for measuring changes in the value of money. Many of the specimens here exhibited are probably new even to specialists. Or if the form was known, its origin and evolution were unknown. Who ever heard, for instance, of Carli and of Dutot as authorities on the subject? The bibliography would alone be sufficient to impart a lasting value to this work.

But Mr. Walsh is much more than a collector of specimens,

[1] *The Measurement of General Exchange-Value*, by Correa Moylan Walsh. New York: Macmillan & Co., 1901.

The powers of a systematic botanist are also his. He classifies the material which he has collected. For example, it is doubtless a great improvement in logical arrangement to distinguish index-numbers in which, as usual, a single system of weights is used for the relative prices, from those typified by Lehr's and Drobisch's methods in which "double weighting" is practised. Again, among methods of weighting each article according to the expenditure thereon, there is a distinction between those which in effect compare the money value of the same set of articles at different times and those typified by Mr. Palgrave's method. I give the essence, as I conceive it, rather than the wording of some passages in the author's learned and logical Appendix C.

Mr. Walsh has not contented himself with classifying the specimens which he has collected. He has also attempted to penetrate to the structure and function of an index-number by a new microscopical analysis. Having observed the properties of the different kinds, by skilfully crossing the "arithmetic" with the "geometric" type he has produced a new variety which may claim to excel in certain respects the existing species.

Limits of space prevent me from tracing these general characteristics through the contents of Mr. Walsh's volume. In truth, it might be feared that my reader's patience would give out if I attempted to reproduce in anything like their original, almost Kantian, elaborateness discussions to which the term "exhaustive," with all its suggestions, is particularly applicable. I will, therefore, select a few points which seem to be of special and permanent interest. Some solid and salient stepping stones may thus be afforded for traversing the flood of dialectic.

Mr. Walsh begins by defining different senses of value. He is specially happy in distinguishing cost value from other species. He complains not without justice, although great names fall under his condemnation, of those who have confounded the different *quæsita*. He well remarks that, if a measure pertaining to cost value is to be constructed, we should not confine our calculations to the consideration of wages, but include profits.[1] His own investigation is confined to "general exchange value," which seems to have a certain parallelism with "final utility," as appears from its relation to Lehr's method :—

"In this method [Lehr's] its author has made an effort to do what appears to be accomplished in the method here presented. He has tried to measure the variation in the average price of

[1] *Cp.* Section on the "Labour Standard" in the Memorandum attached to the third Report of the British Association Committee (above, p. 293).

mass-units, in all the classes, that have the same exchange-value over both the periods together—to which equivalent mass-units he has given the not inappropriate name of pleasure-units" (p. 386).

But Mr. Walsh's exchange-value is more objective (9). The properties of general exchange-value are set forth in a series of propositions, which may deserve the epithet "expletive," in so far as they are mostly self-evident yet render our instructive knowledge fuller and clearer. Among original points may be noticed the distinction between the exchange-value of a thing (*e.g.*, money) in relation to all *other* things, and in relation to all things *including itself* (13). When first the reader learns that exchange-value is considered as objective, he may be disposed to expect that it is an affair only of ratios abstracted from the quantities produced and consumed. Insensibly, however, as we ascend the gentle steps formed by the series of more or less "expletive" propositions, there is borne in on us the need of weighting. We dimly descry a unit, sometimes called an "economic individual" (102, 301), an "exchange-value quantum" (302); we are directed to contemplate "mass-units ideally constructed" (285), "considered as equal, not as weights or capacities, but as exchange-values" (284), in relation to which it is sought to determine the value of money at different times (and places). The data for this determination are prices and quantities of commodity; the problem is properly to combine these data. Two main questions arise:—What importance or "weight" is to be assigned to each of the given prices which enters into the combination? and What should be the method of combination? These questions are first considered separately as far as possible, and then in their necessary connection. I will not follow the preliminary separate inquiries through the windings of Mr. Walsh's exhaustive discussion. Suffice it to notice that materials are not to be included in our index-number along with finished goods (78, 96), apparently for a reason usually given, that the factors of production are counted in the products. Nor is it the quantity of each exchangeable thing that is actually exchanged for money (85), but rather, as I understand, the quantity that is used, which concerns us. As to the method of combining the data we are practically restricted to the three classic Means, the Arithmetic, Harmonic, and Geometric. The author compares the properties of these means, showing certain grounds for the preference of the Geometric:—

"If the exchange-value of money in [B] rises by more than 100

per cent. the compensatory fall of the exchange-value of money in [A] should be to below zero according to the arithmetic method of averaging, which therefore is inapplicable in this case [where [A] and [B] are two equally important classes of things.] And if the exchange-value of money in [A] falls to less than half, the exchange-value of money in [B] should rise from below zero, according to the harmonic method of averaging, which therefore is inapplicable here. But in the use of the geometric compensation there are no such impossible cases" (249).

This passage illustrates certain properties of the compared means, to which the author attaches importance. In the simple case of two extremes, between which a Mean is taken, the distance of the Arithmetic Mean from one extreme, per cent. of the Arithmetic Mean, is equal to the distance of the Arithmetic Mean from the other extreme, per cent. of the Arithmetic Mean. The distance of one extreme from the Harmonic Mean, per cent. of that one extreme, is equal to the distance of the other extreme from the Harmonic Mean, per cent. of that extreme. The distance of one extreme from the Geometric Mean, per cent. of that extreme, is equal to the distance of the Geometric Mean from the other extreme, per cent. of the Geometric Mean. This last proposition cannot be extended from the case of *two* to that of many variables, from the geometric *mean*, in Mr. Walsh's very peculiar phraseology, to the geometric *average*. To the same class of properties, true of the "mean," but not the average, belongs the following, which Mr. Walsh considers important :—

$$\text{If } a_1 a_2 = b_1 b_2, \text{ then } \sqrt{\frac{a_2}{a_1} \times \frac{b_2}{b_1}} = \frac{a_2 + b_2}{a_1 + b_1}.$$

Confining myself to the general and concrete case of plural data, I hasten on to the latter stages in which the question of weights and means, at first separated, are considered in their real connection. We have now to consider penultimately the two simplified cases in which either (1) the sums of money expended on each commodity remain constant at the two periods (or places) compared, or (2) the quantities of each commodity are thus constant; and finally (3) the general concrete case in which both expenditure and quantities vary. In the first case I think most people would be disposed to answer off-hand that the sums supposed constant form the proper weights for an arithmetic combination. The author, however, seems to rightly judge that the ideal of comparing the money-values of the same number of exchange units or "economic individuals" would not be

realised by this procedure; for a reason which he thus assigns with respect to the proposal of taking the arithmetic mean of the sums when supposed different :—

"If it happens that the exchange-value of money has fallen or prices in general have risen, greater influence upon the result would be given to the weighting of the second period. . . . Or in a comparison between two countries greater influence would be given to the weighting of the country with the higher level of prices. But it is plain that *the one period or the one country is as important in our comparison between them as the other, and the weighting in the averaging of their weights should really be even*" (105).

To avoid the difficulty thus indicated, the following formula is proposed in the case of constant sums being expended on each commodity. Let $a_1, a_2$; $\beta_1, \beta_2$; . . ., be the prices at the first and second epoch respectively, and $x_1, x_2$; $y_1, y_2$; . . . the corresponding quantities of commodity; the required index-number is $\frac{x_1\sqrt{a_1a_2}+y_1\sqrt{\beta_1\beta_2}+\cdots}{x_2\sqrt{a_1a_2}+y_2\sqrt{\beta_1\beta_2}+\cdots}$; or, as by hypothesis $x_1a_1 = x_2a_2$, this may be written, $\frac{a_2\sqrt{x_1x_2}+\beta_2\sqrt{y_1y_2}+\cdots}{a_1\sqrt{x_1x_2}+\beta_1\sqrt{y_1y_2}+\cdots}$ (310). The transition is easy from this formula, "Scrope's emended method," as Mr. Walsh calls it, to Scrope's method pure and simple, which is proper to the second abstract case, in which the *quantities* of each commodity are constant, say $x$, $y$ $\cdots$ We have only to substitute in the last written formula, $x$ for $\sqrt{x_1x_2}$, and so on (360). These prolusions lead up to the general concrete case in which neither the sums nor the quantities remain constant. Guarding against the difficulties encountered in the simpler cases, the author proposes this "universal formula":—

$$\frac{x_2a_2+y_2\beta_2+\cdots}{x_1a_1+y_1\beta_1+\cdots}\times\frac{x_1\sqrt{a_1a_2}+y_1\sqrt{\beta_1\beta_2}+\cdots}{x_2\sqrt{a_1a_2}+y_2\sqrt{\beta_1\beta_2}+\cdots}.$$

This form is shown to have a certain theoretical advantage over other species of index-number, in particular those which, as affected with "double weighting," most challenge comparison with it, namely Drobisch's and Lehr's methods. The universal formula satisfies *some* of the criteria which Mr. Walsh has laid down. It does not, however, in general, satisfy what he has called Professor Westergaard's test that (*e.g.*) "prices measured from 1860 to 1870 and from 1870 to 1880 ought to show the same variation from 1860 to 1880 as would be shown by comparing

the prices of 1880 directly with those of 1860" (205). One may imagine a world in which the universal formula, and even "Scrope's emended method," would completely satisfy Professor Westergaard's test and all other tests. "But in the world as it is, we have not reached the absolutely true method" (402).

What now is the worth of this result and of the investigations which lead up to it? The answer to this question will vary with the critic's preconceived opinion on some very debatable first principles. I, for one, find myself at variance with Mr. Walsh on certain fundamental issues, for the discussion of which I have thought an independent article more appropriate than a review.

I cannot accept a view of the subject according to which it is significant to seek an exact measure of the change in the value of money in the case where only *two* relative prices are given. This paucity of data would indeed be innocuous if we had as clear and objective a perception of the units of exchange-value as of the units of mass and motion, or the degrees of the thermometer. On that supposition we might even speak with Mr. Walsh of obtaining an expression for the "general exchange-value" of money, or any one thing, "at each period separately" (76, *cp.* Appendix A). A series of such expressions for successive years would no doubt satisfy Professor Westergaard's criterion above mentioned and all other tests. But I can form no idea of such a general exchange-value, except the somewhat indefinite notion of the relation between an amount of money and the quantity of utility which it will procure. I have not the courage to speak with Professor Irving Fisher of a *util* as an hedonic unit, I do not insist on the term utility, but only on the fact that our perceptions of the value of money in relation to such a unit as is desiderated are vague and indefinite. Suppose that one large class of commodities, say those following the law of decreasing returns, were to rise in price each by the same or nearly the same percentage, while all other articles in use, also forming a large class, were to fall together; that in such a case the exchange-value of money has varied by so much would appear to me a somewhat indefinite proposition—its subject deficient in logical clearness, and its predicate in numerical precision. On such a supposition the objections which have been urged by a distinguished economist against index-numbers,[1] that the results are widely different according as different species of averages are employed, would seem to me a fatal objection. The wide differences which may exist in such a case between different means are indeed of a piece with the enormous discrepancies which might be expected between the estimates

[1] Economic Journal, Vol. VI. p. 130.

of equally competent judges as to the change in the value of money in respect to some such unit as it postulated. For example, if the drop in one large class, including necessaries, was great, while the rise in the remaining class was small, it would probably seem to all that money had fallen in value; it might seem to only a few that it had fallen to half its original value; but between these limits there might be no unanimity. With all his logical precision, Mr. Walsh does not seem to have removed what Mill calls "the necessary indefiniteness of the idea of general exchange-value." Mr. Walsh admits that "we have not yet reached the absolutely true method." I am disposed to think that we never will reach an exactly true method on his lines, until we are able to handle and weigh final utility, or what he calls "esteem-value," as we do material commodities.

What should we think of a book which purported to instruct the Civil Service Commissioners who superintend our public examinations as to the principles by which their judgment should be decided in cases where there might be only two marks for each candidate, say one in literature and one in science? Should we expect that any skilful blend of arithmetic and geometric mean would bring out a true figure, representing the real relation between the merits of the candidates? That large part of Mr. Walsh's analysis which is devoted to the case of *two* data appears to me to be equally foredoomed to failure. I should not expect much useful suggestion from any formula which holds good only for the artificially simplified case of dual data, and not for the concrete reality of plural data.

Doubtless a certain interest is excited by this attempt to feel after a conception of general exchange-value. Perhaps posterity will regard these tentatives as we regard the exercise of thought by which appropriate conceptions in mathematical physics have been won. Or, to compare small things with great, the better parallel might be found in the disquisitions by which the ancient philosophers made familiar, if they did not make quite definite, many abstract terms which are still in use. Meanwhile our author has a less pleasant feature of resemblance to the Greek sages, namely a proud confidence in dialectic, to the neglect of more positive science. I refer to his treatment of the Calculus of Probabilities. He regards it as irrelevant (38,) and takes Cournot to task for applying it to the problem in hand (38, 66, 69). This omission of Probabilities appears to me serious. Even granting [1] that the *primary* problem is to measure the value of money in some

[1] Without prejudice to the claims of the "Labour" or "Real Cost" standard; which we may agree to postpone as not ripe for discussion.

such unit as Mr. Walsh desiderates, still by rejecting the Calculus of Probabilities he has not only thrown away an instrument necessary for the performance of that measurement, but also has lost sight of an important *secondary* aspect of the problem.

First, according to the view here submitted, the estimate of the relation between money and the unknown unit based upon one or two price variations is very vague—the discrepancy between equally authoritative estimates might perhaps be as likely as not to amount to twenty-five per cent. in accordance with the suppositions made just now. But by the Theory of Probabilities, as observations are multiplied, the enormous "probable error" incident to the individual observations becomes diminished in the average. The rope is much stronger than its component strands. I would not deny that there is some philosophical difficulty in thus obtaining a definite measurement of a quantity, the degrees of which are not capable of being perceived distinctly. Rather, I would say with Professor Marshall,[1] that an absolutely perfect standard is "unthinkable." But here, as in wider spheres of conduct, although speculative difficulties cannot be perfectly resolved, we may obtain sufficient guidance for action. One useful direction is that "weighting" is of less importance than at first sight appears. Even with reference to what I am willing to regard as the primary *quæsitum*, it is safe to say with Mr. Bowley that "no great importance need be attached to the special choice of weight."[2] It is well to imitate the judicious compromise and happy ambiguity of Sir Robert Giffen in the second Report of the British Association Committee (1898):—"Practically, the Committee would recommend the use of a weighted index-number of some kind, as, on the whole, commanding more confidence. But they feel bound to point out that the scientific evidence is in favour of the kind of index-number used by Professor Jevons—provided there is a large number of articles—as not insufficient for the purpose in hand. . . . A *weighted* index-number, in one aspect, is almost an unnecessary precaution to secure accuracy, though, on the whole, the Committee recommend it."

I do not retract the opinion which has been expressed above that the index-number elaborated by Mr. Walsh[3]—the one applicable to the general case of varying quantities and prices—has a certain theoretical advantage over its predecessors. But I doubt whether the advantage of this method over the simpler method sanctioned by the Committee of the British Association

[1] *Contemporary Review*, 1887.

[2] *Elements of Statistics*, p. 113; *cp.* ch. ix. 1.

[3] Above, p. 373.

is so great as to compensate the trouble of applying the more complicated method. This doubt is confirmed by the following consideration. It seems to be admitted by high authorities—and Mr. Walsh would apparently agree [1]—that the most exact solution of the concrete problem is obtained by a series of index-numbers taken at short intervals of time. Now the interval of time between any two adjacent index-numbers being small, we are entitled to assume that the change in price and also in quantity during any such interval is small. Accordingly let us substitute in Mr. Walsh's above-written formula for $a_2, \beta_2, a_1 + \Delta a_1, \beta_1 + \Delta\beta_1$, and similarly for $x_2$, $y_2$, $x_1 + \Delta x_1$, $y_1 + \Delta y_1$, where $\Delta a$, $\Delta\beta$, $\Delta x_1$, $\Delta y_1$ are small (relative to $a_1$, $\beta_1$, $x_1$, $y_1$ respectively), in such wise that the second and higher powers of the quantities $\frac{\Delta a_1}{a_1}$, $\frac{\Delta x_1}{x_1}$, etc., are small fractions. Then, expanding in powers of $\Delta a_1$, etc., $\Delta x_1$, etc., we find that Mr. Walsh's "universal" formula differs from the index-number recommended by the British Association Committee only by quantities of the *second* order. It may be added that the elegant formula which, as above mentioned, Mr. Walsh introduces as "Scrope's emended method" differs from the index-number of the British Association Committee only by quantities of the *third* order.

Mr. Walsh seems to have exaggerated the need of weighting. He gives the *Economist's* index-number as an example of the discrepancy resulting from different weights (83).

"In the comparison given by Mr. Palgrave of the Economic series of 'unweighted' index-numbers and the index-numbers calculated upon the same prices, we find the following contrasts :—

| | | |
|---|---|---|
| 1880 | 87 | 89 |
| 1881 | 81 | 93 |
| 1882 | 83 | 87 |
| 1884 | 79 | 88 |

Here the calculated movements of general prices go in exactly opposite directions in every sequence of years. Between the first and the second years, for instance, the *Economist* figure falls 7 per cent., and the 'corrected' figure rises 4½ per cent.—a difference of 12 per cent. Divergences of this sort are to be seen in every case where in a series of periods the same price has been treated in both ways for comparison."

But in a matter of this sort we should look to the average

[1] P. 113, referring to the Report of the British Association for 1887, our first Memorandum, above, H.

character of experience rather than at exceptional instances. The rudimentary index-number of the *Economist* appears less typical than Mr. Sauerbeck's index-number or that compiled by the Aldrich Report,[1] each of which gives almost identically the same result whether unweighted or weighted. We should contemplate in the statistics compiled by the Bureau of Economic Research,[2] the curves which represent the weighted or unweighted index-numbers hugging each other closely through the long course of years. We should take into account too the *a priori* reasons for expecting this sort of correspondence, reasons which derive some confirmation from their verification in the like matter of wage statistics. See the "example of the smallness of the change introduced by difference in systems of weighting" in Mr. Bowley's *Elements of Statistics* (p. 114 *et sqq.*, cp. *ibid.*, p. 219, "On the unimportance of weights," *et sqq.*).

Doubtless divergences of the sort, to which our author points triumphantly, "are to be seen in every case" if you look out for them; just as extraordinary sequences are to be seen in games of chance if you look out for them long enough. Mr. Walsh, indeed, has not been very happy in his selection of a specious exception. By a pardonable oversight it has escaped his attention that the index-numbers which he contrasts are not as he supposes "calculated upon the same prices." The unweighted index-number is taken from Mr. Palgrave's Table 26,[3] in which the prices of *cotton-wool, cotton-yarn, cotton-cloth,* play a part. The weighted index-number is taken from Mr. Palgrave's Table 27, from which these three prices are excluded. For the purpose in hand it would have been proper to exclude those three cotton prices, as is done in the Memorandum attached to the Second Report of the British Association Committee. I reproduce the result so far as relevant here.

| | 1880. | 1881. | 1882. | 1883. |
|---|---|---|---|---|
| Mr. Palgrave's Weighted Mean for 19 articles ........................................ | 89 | 93 | 87 | 88 |
| The simple Arithmetic Mean for the same articles ........................................ | 93·5 | 86 | 89 | 85·5 |
| Excess of Arithmetic over Weighted Mean | 4·5 | − 7 | + 2 | − 2·5 |

[1] See ECONOMIC JOURNAL, Vol. VI. p. 136. [2] *Ibid.*, Vol. X. p. 600.

[3] Third Report of the Royal Commission on Depression of Trade and Industry. [*C.*—4797], 1886; pp. 343—353 (cp. *Brit. Ass.*, 1888, p. 203).

It is still true that "the calculated movements of general prices go in exactly oppôsite directions in every sequence of years," that is three times.[1] But as the distance to which they go is inconsiderable in comparison with the "probable error" to be expected, it would be requiring too much that they should always go in the same direction. The figures in the table from which an extract is given had been noticed in the Memorandum referred to as exceptional, not on account of their divergence but on account of their agreement. "The annexed comparison," it was there remarked, "does not present the appearance of pure chance. The discrepancies are rather *less* in magnitude than the theory regards." This "faultily faultless" character of the index-number is *pro tanto* corrected by Mr. Walsh when he points out some little discrepancies in the matter of the sequences.

Had he bestowed more attention on the theory of averages, our author would have asserted with less confidence that "in no other case [except the case in which all prices vary alike] do we want to seek any determination 'irrespective of the quantities of commodities.'"[2] There is a *secondary* form of the problem with respect to which weighting has even less importance than under the first aspect. I may introduce this variety by a problem which has been likened to the problem now before us, the determination of the sun's motion relatively to the sidereal system. Referring to this sort of problem Mr. Walsh has some just remarks on the relative motion of the single body and the system (68, *cp.* 38). He may be right in suggesting that the use of Probabilities in the analogous monetary problem has sometimes been connected with a confusion between cost-value and the kind of value which he has set himself to measure. Yet I do not feel sure that the function of the Calculus is adequately recognised in the following passage :—

"When we have chosen which method we shall adopt, and what shall be our standard [whether we shall consider motion of a body relatively to *all other* things, or to *all* things including itself],

[1] Out of fifteen sequences or changes from year to year shown by the complete table *eleven* are in the same direction for both weighted and unweighted index-numbers; *four* are in opposite directions, viz. 1873–1874 and the three sequences selected by Mr. Walsh, 1880–1881, 1881–1882, 1882–1883.

[2] Page 222, note. Referring to the present writer's Memorandum attached to the Report of the British Association Committee, 1887, p. 280; where the commentator strangely supposes that the case contemplated is that "in which all prices vary alike." The context of the section referred to and the parallel section in the third Memorandum (Report of the British Association, 1889, p. 156) make it clear that the sought common effect of changes in the supply of money is not supposed to be given free from disturbances special to particular commodities (*cp.* below, p. 380 *et seq.*).

there is of course no occasion for employing in our measurements the law of probabilities—as was asserted also in this connection by Cournot. We do not say it is more probable that all the other things have remained stationary than that this one has stood still and they moved; or it is more probable that all things have together remained stationary, wherefore both this and the others have moved relatively to the whole. But having adopted our point of view we simply measure as best we can what we see happening before us. And our point of view itself in these matters we adopt not by any use of the law of probabilities, but because the myriad inter-relations which do not change, or which do not change on the average, make more impression on us than the particular ones which do change" (69, 70).

However this may be, it does not invalidate the proposition which I am concerned to maintain: that without knowing the centre of gravity, or "weighted mean" of a system of bodies, we may know by the theory of averages that one single body is advancing through the cluster. Leaving the problem of the stars, which involves some technicalities, let me take a humbler terrestrial illustration. The annexed pairs of figures were thus obtained: As I walked along Piccadilly one day I noted the number of omnibuses * which met me (viz. 7) and the number which passed me (viz. 3) out of the first *ten* which came up to me, whether they were moving in the one direction or the other; and so on for successive decades (the observations not being all made on the same day, nor at the same hour). Here are some of the observations:—

7, 3; 8, 2; 8, 2; 5, 5; 7, 3; 8, 2; 7, 3; 6, 4;
7, 3; 6, 4; 7, 3; 7, 3; 6, 4; 8, 2; 8, 2; 7, 3;
8, 2; 4, 6; 7, 3; 7, 3; 8, 2; 6, 4; 9, 1; 8, 2.

From these and other observations in *pari materia*, I find that on an average of the omnibuses observed, about 70 per cent. met and 30 per cent. passed the observer. If, as there is reason to suppose [1] (at the hours when the observations were made), the

* The vehicles were drawn by horses in those days. The experiment recently repeated with respect to motor-buses gave a different result.

[1] This presumption is confirmed by the following statistics in which the first member of each pair (*e.g.*, 6 in the first pair) denotes the number of omnibuses moving eastward, and the second number (*e.g.*, 4 in the second pair) denotes the number moving westward, out of every ten omnibuses, which, sitting at the window of a club in Piccadilly, I observed passing in either direction:—

6, 4; 5, 5; 4, 6; 5, 5; 6, 4; 6, 4; 5, 5; 3, 7;
5, 5; 6, 4; 3, 7; 5, 5; 5, 5; 7, 3; 5, 5; 5, 5.

It may be noticed that on the basis of the calculation in the text the observer

same number of omnibuses are moving in both directions with the same average velocity, say, $V$; an easy calculation shows that the velocity of the pedestrian, supposed uniform, $= (0{\cdot}7 - 0{\cdot}3)V$, $= 0{\cdot}4V$. That is the absolute velocity, so to speak, referring, say, to some fixed point in the street. Accordingly the velocity of the pedestrian relative to the vehicles which are moving in an opposite direction to his is $1{\cdot}4\ V$: and relative to the vehicles which are moving in the same direction, $\cdot 6\ V$. If, then, the pedestrian could observe his own velocity relative to a great number of vehicles taken at random from the whole series—say all that at a given instant were in Piccadilly—the distance by which he would be found to gain upon the average omnibus in a unit of time would be about $(1{\cdot}4 - \cdot 6)V = \cdot 8\ V$. This datum might possibly have been obtained by observation, if the observer had attended to the relative velocities of the vehicles in his neighbourhood, not merely to the numbers which met him and passed him, as he walked.

The distance which the individual on foot moves relatively to the average omnibus during a unit of time may be treated as a substantive entity, an independent measure of the rate at which the individual is advancing through the crowd of vehicles. Or it may be regarded as an approximation to a perhaps more scientific *quæsitum*, the rate at which the individual is moving towards the *weighted mean* of the system. The simple average might be used for this ancillary purpose by one who had not the means of ascertaining the centre of gravity of the system, or even by one who had not formed a very clear idea of what is meant by a centre of gravity. The approximation may be expected to be very close. For the statistics now under consideration are simply related to the group above cited, representing the proportions of vehicles meeting and passing the pedestrian; and this group appears to possess the characteristic on which indifference of weighting depends, namely, *sporadic dispersion* about a constant mean.

Is it necessary to interpret the parable? The oscillating crowd of public conveyances is comparable to the long list of commodities with ever varying values—the swaying series of the logarithms [1] so taken that the difference between any two of them represents the relative value of two articles of exchange. The

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would appear to be moving westward with a velocity equal to an *eightieth* of the average velocity of an omnibus; a result which differs from zero by an amount which is well within the probable error incident to the calculation.

[1] As conceived by Cournot (*Théorie Mathématique des Richesses*, ch. ii.); who very properly in this connection does not mention *weights*.

change in the distance of the pedestrian from the "weighted mean" of the system represents the *primary* monetary *quæsitum;* the change in his average distance from the other bodies in the system represents that unweighted—that is, equally weighted, or more generally randomly weighted—mean of relative prices, which may be used either as subsidiary to the primary investigation, or as an independent *secondary* measure. The position of high collateral dignity is all the more deserved in that the secondary measure enjoys an objective or external character, which cannot—according to my view of the subject—be accorded to the primary *quæsitum.*

The recognition of this sort of absolute standard, or at least of that sporadic dispersion on which it is based, demands a considerable widening of the views and softening of the strictures, which we find in the work before us. First, more attention may be claimed for a species of average, appropriate to the secondary *quæsitum*, the *Median*, which Mr. Walsh has mentioned only to reject. Again, his criticism of those who have sought to include wages with commodities in an index-number seems too harsh. Those certainly are to be condemned who confound the distinct standards, which are based on the amount of commodity which the same sum of money will procure, and the amount of effort and sacrifice which are required to procure the same sum of money. Mr. Walsh is quite justified in describing a mixture of these two species of index-number as an unmeaning "hodge-podge." But there is a secondary point of view in which these distinctions are less important: the view which seems to have been taken by some of the great men who first approached our problem. When Hume imagined every one awaking one morning with an additional coin in his pocket, when Mill improved on the idea by imagining the money in every one's pocket to be increased in a certain ratio, presumably they thought of prices in general without distinction of producers' and consumers' goods. And certainly in an alert state of competition, if such a change as Jevons proposed for the purpose of unifying international coins were carried out, namely that what is now 100 dollars should reckon as $103\frac{1}{2}$, it is very conceivable that this change would rapidly propagate itself through a great variety of transactions, including those between master and servant. And accordingly, though the change in wages in each department might be liable to the same proper disturbance as the finished article (in addition to the common monetary influence), and so far as they are not *independent* observations it would not be much good including

them, at the same time there would be no harm in including them in such an unweighted index-number as is now under consideration. I am not contending that wages ought in the existing state of things to be included in any kind of index-number along with finished products. I am only regretting that our author's great learning has not saved him from the common defect of original writers on the subject, an inability to perceive the many-sidedness of the problem, an exclusive devotion to one idea.

There are more things in the monetary cosmos than are dreamt of in his philosophy. Still his philosophy is of a very high order. So subtle dialectic, such logical precision, supplemented by a diligence of literary research that is quite unrivalled, if brought to bear on other economic problems, may be expected to merit a less chequered encomium. That they have not now obtained a more decided success seems due to the peculiarity of a problem which involves the more positive science of Probabilities. But, I repeat, this is an individual opinion on a much debated question. There are those who conceive the problem in a sense more favourable to Mr. Walsh. To me he seems unfortunate in his subject; to others perhaps, only in his critic.

## (N)

# PROFESSOR WESLEY MITCHELL ON INDEX-NUMBERS

[In this paper, published in the Economic Journal, 1918, under the title "The Doctrine of Index-Numbers according to Professor Wesley Mitchell," the plurality of conceptions attached to the term change-in-the-value-of-money, the variety of purposes subserved by an index-number for prices, is urged once more and with more confidence than before. That an index-number may be more than a register of a change in the value of certain specified articles, that there is an average trend of prices which may be expressed by methods other than those of a commercial account—this view is more acceptable now than it was thirty-five years ago. It is recognised in theory by Professor Mitchell, and realised in practice by Mr. Flux.]

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The problem of which the object is to measure changes in the value of money has long exercised economists and statisticians. Thirty years have elapsed since the British Association appointed a committee for the purpose of investigating the best methods of ascertaining and measuring variations in the value of the monetary standard. The wording of this instruction may serve to remind us of the tremendous magnitude which the phenomenon to be measured has since the outbreak of war assumed. No one would now set out to ascertain the fact of a change in the value of money—a fact which in the peaceful eighties of last century could be disputed by sturdy mono-metallists without obvious absurdity. But though the fact now stares us in the face, the measurement of its magnitude is still important; perhaps more important than ever. For it can hardly be doubted that as the war goes on, and during the period of so-called "reconstruction," there will be required careful measurements of change in the purchasing power of money, with a view to the adjustment of wages and of other payments. And not only for practical purposes, but also in the interest of monetary theory, will

such measurement be urgently required. In the controversies which will probably flourish in the early part of the twentieth, as in that of the nineteenth, century concerning the management of the currency during a great war, reference will certainly often be made to the index-numbers which represent the change from time to time in the level of general prices. If, as may be expected, the quantity theory of money is appealed to, it will be proper to construct another kind of index-number showing changes in the volume of trade. And other index-numbers there are which may be required in the course of reconstruction; in particular, those which measure wages nominal and real.

Coincidently with the increased demand for the use of index-numbers it is opportune that there has appeared a singularly comprehensive and lucid treatise on this species of measurement.[1] It is true that Professor Wesley Mitchell's monograph on index-numbers of wholesale prices does not cover all the ground which we have here in view. But the methods appropriate to the general problem can mostly be learnt from his discussion of a particular but leading case. That discussion is so complete and thorough that it almost dispenses the student who is not a specialist from the trouble of consulting the earlier literature of the subject. Within a limited but considerable and representative province Professor Mitchell has explored every inch of the ground. He has traced the many-branching paths which perplexed most of his predecessors. He has added clear directions showing where each of the paths leads.

The last-mentioned task is more difficult and important than may be supposed. It is a peculiarity of the problem that much thought must be expended in order to find the meaning of the question before you begin to answer the question. The practical man intent upon making or spending money does not suspect the ambiguity lurking under inquiries about its value. He asks what is the equivalent in our currency of the guinea in Charles II.'s time, and expects an answer as pat as if he had asked what is now the bank rate, or what the price of wheat. It is true that where the distance between the epochs compared is not so enormous, in the more usual comparisons of price-levels, the definition of the question is not so important; much the same answer may be given to different varieties of the question. The relation is like that between ethical theory and good conduct; if Bishop Butler and other moralists are right in thinking that

[1] Index-numbers of wholesale prices in the United States and foreign countries (*Bulletin of the United States Bureau of Labour Statistics*, 1915; whole number 173).

much the same conduct may follow from first principles so opposite as rational benevolence and rational self-love. When this analogy was suggested to Sidgwick, on the occasion of a meeting of the above-mentioned British Association Committee, the author of the *Methods of Ethics* made reply to the effect that, while frequently different methods might be adopted without obvious difference in practice, yet occasionally at critical turning-points the difference between opposite first principles would make itself felt decisively. We surmise that in like manner monetary distinctions which are otiose in ordinary times may have become significant under present conditions. All the greater is the debt of the economist to Professor Mitchell for having made the distinctions clear. Consider, for instance, Professor Irving Fisher's index-number in which each article is weighted in proportion to the number of times it is sold; quite properly, as Professor Mitchell points out (78),[1] with reference to Professor Fisher's purpose. In ordinary times there would probably be little difference between this number and that which is obtained by using the same commodities in the same quantities without taking account of the number of turnovers (Mitchell, *loc. cit.*).[2] But in war time, methods of business being considerably altered, it is possible that the distinction corresponds to a real difference.[3] The same may be said about another variety which Professor Mitchell thus distinguishes. "If the aim be merely to find the differences of price fluctuation characteristic of dissimilar groups of commodities, or to study the influence of gold productions, or the issue of irredeemable paper money upon the way in which

[1] The numerals in brackets refer to pages in Prof. Mitchell's treatise.

[2] Cp. *Memorandum British Association Report*, 1889. Professor Foxwell's method, above, p. 261.

[3] The subtlety of these distinctions deceives even experts. Thus the reviewer in the *Economist* (for January 12, 1918) criticising a recent publication in which it was held not to be proved, upon the lines of Irving Fisher, that money rather than goods was responsible for the rise of prices, observes triumphantly: "There can be no question that the increase in currency has been very much more rapid than the increase in the production of goods, unless we are to assume that this country, with four or five millions of its best men withdrawn into the Army, has been able to increase its production by more than 50 per cent." But the four or five millions have *not* been withdrawn from the production with which we are concerned in *this* inquiry. They (with their dependents) make, *primâ facie*, at least as great a pull as before upon the currency. Again the increase of women's and old men's paid work swells the denominator, which Irving Fisher calls "T." But then, asks the reviewer, why have prices risen? Quite conceivably, we reply, not so much because the quantity of money has increased out of proportion to the quantity of "goods" (in the sense here relevant), as because the circulation of the goods is less rapid (as suggested in the work criticised [*cp.* Lehfeldt, ECONOMIC JOURNAL, Vol. XXVIII. (1918), p. 111]). That is not "inflation" in the sense of causation on the side of money.

prices change, it may be appropriate to give identical weights to all the commodities " (78).[1] Again, the consumption standard, as based on family budgets, or more generally on the expenditure of the citizens in the way of consumption for the sake of personal or sympathetic satisfaction, exclusive of their collective expenditure on munitions for the satisfaction of patriotic motives, may well differ in war time from an index-number like that of Professor Irving Fisher, if there is included in the work which the currency has to do the payments by the Government for munitions. Conceivably, however differently from present experience, the *momentum* (price $\times$ velocity) of currency in relation to the " volume "—or, rather, the momentum, or *flow*—of goods, including munitions, might remain constant; while the prices of all the goods consumed by the citizen, exclusive of munitions, rose considerably.

Our readers are perhaps beginning to feel that they have had enough of this concept-splitting. Yet there remain certain varieties of index-numbers which we cannot pass over: two mentioned by Professor Mitchell and two which it did not come within his subject, more narrowly defined than ours, to mention. There is first the index-number intended to serve as a business " barometer " (66). If the aim be to construct a business barometer, the data should be prices from the most representative wholesale markets, the list should be confined to commodities whose prices are most sensitive to changes in business prospects and least liable to change from other causes, and the weights may logically be adjusted to the relative importance of the commodities as objects of investment. Professor Mitchell also directs attention to what he calls a " general-purpose " index-number, not adapted to any special end and in practice applied to very various purposes, of which more than a dozen are enumerated (26).[2] Professor Mitchell is no doubt right in thinking that " the day has not come when the uses of index-numbers are sufficiently differentiated and standardised to secure the regular publication of numerous special-purpose series." Till then " the users of index-numbers must put up with figures imperfectly adapted to their ends " (26).

[1] Cp. *British Association Memorandum,* 1887, section viii.: " Determination of an Index-number irrespective of the quantities of the commodities."

[2] Cp. *Memorandum,* 1887 (above, p. 255), " mixed modes, compounding the ends or means or several distinct methods " . . . " the most comprehensive . . purporting to be a compromise between all the modes and purposes—*the* method if practical exigencies impose the condition that we must employ one method, not many methods."

Another conception of the end, another definition of the value of money, is derived from Ricardo's axiom that "a commodity which at all times requires the same sacrifice of toil and labour to produce it is invariable in value." Professor Marshall has countenanced this view of our problem. In his evidence before the Precious Metals Royal Commission of 1888, speaking of the appreciation of gold,[1] he said: "When it is used as denoting a rise in the real value of gold, I then regard it as measured by the [increase] [2] in the power which gold has of purchasing labour of all kinds—that is, not only of manual labour, but the labour of business men and all others engaged in industry of any kind." It has been said that changes under this head are sufficiently reckoned with when the changes in average incomes are noted. This, however, may be questioned in time of a war involving enormous changes in the quantity of labour employed in production, additions here and subtractions there.

Nor can we pass over in silence Professor Nicholson's index-number based on capital.[3] It is remarkable that the conception which lies at the root of this method should have been that which, under a different aspect, first presented itself to Professor Lehfeldt in his independent and original investigation of the "absolute price of gold." [4] Professor Lehfeldt's *second* definition, referring to a "redistribution of effort of production" on the supposition of "the total of effort being unchanged," savours of the labour standard which we mentioned just now.

When we have decided what is the end at which to aim, we may go on to consider how the data are to be shaped to that end, and what data are to be sought. The step which is last in the analysis, as Aristotle would say, is first in the order of practice. The initial operation of collecting the original quotations of price requires more care and labour than might be supposed. "To judge from the literature about index-numbers, one would think that the difficult and important problems concern weighting and averaging. But those who are practically concerned with the whole process of making an index-number from start to finish rate this office work lightly in comparison with the

[1] Appendix to Final Report [C 5512] Question 9025. Quoted in the *British Association Memorandum* of 1889, p. 161.

[2] "Diminution" has been substituted for "increase" in the original by an obvious misprint.

[3] Described in the *British Association Memorandum*, 1887, section vi., above, p. 230.

[4] Economic Journal (March, 1918, p. 108).

field work of getting the original data" (27).[1] The fathers of the English Statistical Society were so apprehensive lest the field work of collecting facts expressed in figures should be neglected if attention were diverted to drawing inferences from those facts that they proposed to divide the two kinds of work, and as the motto which they chose purported—*aliis exterendum*, surmounted by a wheatsheaf—themselves to gather in the harvest of statistics, while leaving it to others to thrash out the inferences. But Professor Wesley Mitchell has shown that it is possible for one and the same individual—combining official diligence with economic subtleties and statistical refinements—both to collect the raw material of primary data, and also to employ the complicated machinery which is required in order to render that material available for human use. We have not space to describe the excellent directions which are given to "the field worker collecting data for an index-number" (27). Indeed the whole of Part II., nearly two-thirds of the volume, dealing with index-numbers of wholesale prices in the United States and foreign countries, abounds with suggestions which may be useful to the practical statistician. Attention should be called to the suggestion that the facts may prove to be of more permanent interest than the theories which are now built thereon. "It is probable that long after the best index-numbers which we can make to-day have been superseded, the data from which they were compiled will be among the sources from which men will be extracting knowledge which we do not know enough to find" (30). We surmise that some of this future knowledge will be of the kind to which Professor Mitchell points: "to find how prices are interconnected, how and why they change, and what consequences each change entails" (29, 67).

Between the collection of the data and the completion of the index-number there are several intermediate processes which Professor Mitchell describes under the headings *base periods, the numbers and kinds of commodities included, problems of weighting, averages and aggregates*. We adopt this division, but we are not careful to follow the author's order as to the topics which are ranged under these four heads.

[1] One who was associated with Giffen when he was preparing the scheme of an index-number adopted by the British Association Committee can remember how much he was influenced in the selection of the items by the possibility of obtaining an available figure. He has himself expressed this in the second Report of the Committee (1888) which he drew up. "In dealing with the question practically those concerned must always have an eye upon the data, and consider what is practically attainable" (*loc. cit.* p. 183, and context).

Under the first head Professor Mitchell's most important contribution is the support which he gives to the method proposed by Professor Marshall, according to which the *base* adopted each year is constituted by the prices of the preceding year. "Chain" index-numbers it is proposed to call this species (36, 37, referring to 23). The ordinary "fixed base" index-number—for example, one constructed for the year 1913 with the prices of 1890–99 as base—is liable to an imperfection which is thus worded: "As the years pass by the commodities that have a consistent trend gradually climb far above or subside far below their earlier levels, while the other commodities are scattered between these extremes. Thus the percentages of variation for any given year gradually get strung out in a long, thin, and irregular line without any marked degree of concentration about any single point" (23). On the other hand, a careful scrutiny of the relative prices with which the "chain" method deals brings out the interesting circumstance that these percentages are grouped approximately according to the "normal law" of distribution. The familiar form which has been likened to (the front view of) a *gendarme's* hat reappears. But it should be noticed that in the centre of the hat there is a spike like that of a Prussian helmet—a "mode" which is very abnormal.

There is something impressive in the introduction of the normal law—the dominant principle of the higher statistics—into questions relating to money and prices. It is like the appearance of a distinguished savant as witness in a case relating to ordinary business. Let us make certain that the testimony is rightly interpreted.

When it is claimed as a merit of the "chain" data that they conform to the normal law, the question arises what advantage is there in such conformity. The feeling of statisticians on this question may perhaps be expressed by the old answer, "Si non rogas, intelligo." To reply that the law is convenient for purposes of calculation seems hardly relevant to the present inquiry. A deeper reason may be found in the presumption that the law is the outcome of numerous independent causes.[1] Since it is unlikely that independent phenomena should vary concurrently, we have here some guarantee of a certain stability in the grouping under consideration. It may be worth suggesting that prices regulated by Government in war time are determined by general

[1] Independence being understood in the sense explained by the present writer. *Journal of the Royal Statistical Society*, 1916, p. 462, and references there given.

rules rather than the plurality of fleeting causes which constitute the condition of the normal distribution. But we are not prepared to affirm that arbitrary governmental regulations will be deficient in the element of haphazard.

However, we do not dispute that it is a merit in a statistical group to conform to the normal law. We admit, too, that a continual elongation in one direction, such as Professor Mitchell has observed in the case of some relative prices, tends to deformation of the law. If *all* the prices behaved in this way, some moving upwards continually, the others downwards, they would

"leave in the midst a horrid vale,"

quite inconsistent with the normal contour. As nothing like this occurs, the continued elongations can be only moderately disfiguring; to what extent must be a matter of observation. We are not satisfied that this observation is performed with sufficient care by Professor Mitchell. He shows two diagrams, one representing the distribution of prices in 1913 as compared by the "chain" method with those of 1912, the other the distribution of 1913 compared with 1890–99 by the "fixed-base" method; and points out that the former set obey approximately the normal law. "But," he continues, "the distribution of the second set of variations (percentages of change from the average prices of 1890–99) . . . belongs to a different type. It has no pronounced central tendency; it shows no high degree of concentration around the arithmetic mean or median. It is more like an oblong than like the bell-shaped normal curve . . . its probable variation is five times as great as that of the corresponding variations for 1912 prices." This evidence is not conclusive; for it may be shown that the same appearance would be presented in like circumstances by the most perfectly normal distribution.

To construct an ideal distribution imagine a game in which each player moves a peg one step of, say, a quarter of an inch on a horizontal board either forward or backward, according as a tossed coin shows head or tail. A number of players that move thus east or west start from a line running north and south. Suppose that each player takes several steps in five minutes (corresponding to several changes in the price of an article during a year). At the end of that period the distances from the initial line will be ranged in at least rough correspondence to the normal law. Now let the race be prolonged for more than two hours—twenty-five periods each of five minutes. At the end of this time the normal distribution will be much more perfect (since

more independent causes will have operated). But appearances would be against the new group. It might be said of it, as of the "fixed-base" series, that "it has no pronounced central tendency; it shows no high degree of concentration around the arithmetic mean," and so forth. For let there be given the "probable deviation" of the grouping after the first five minutes. Say it is so many quarters of an inch, or, better, a percentage of a certain standard length; namely, the number of inches which measures the distance of the initial line—the "carcer" of our imagined race—from a zero-point (west of that line). Let the said probable deviation be 4 (per cent.). Then the probable deviation for the group at the end of the race will be *five* times as great,[1] namely, 20. The latter grouping naturally appears "oblong," as contrasted with the "bell-shaped" contour of the former. The contrast is exhibited in the accompanying diagram,

| | | | | | |
|---|---|---|---|---|---|
| 16 | 38 | 60 | 60 | 38 | 16 |
| 14 | 15 | 15 | 15 | 15 | 14 |

where the notches on the line are placed at intervals of 4 (per cent. of the standard length), the probable deviation of the grouping formed by a five minutes' race. The upper figures show roughly (part of) a group of 240 observations thus formed. The lower figures show the grouping that may be with most probability expected after the forces tending to dispersion have acted for twenty-five times five minutes. It will be noticed that the shape of the bell is no longer conspicuous, about the centre at least. Certainly, if we exhibited the whole group, it might come out; it would come out if we represented, not the result of one race, but the average result of indefinitely numerous trials. But at any one race of the kind which we have described the grouping as a whole would assuredly appear rather "oblong." And yet it may be more normal than the small group. If the distance of each player from the starting-point at the end of the long race is divided by 5, the group so formed may be expected to comply with the ideal shape better than the set of points reached in the short race.

This contrast is not materially affected by the introduction of certain concrete circumstances. The coins which are used might be slightly unsymmetrical. There would then result a grouping which has many properties in common with the normal shape, the sub-normal curve as it has been called.[2] The sub-

[1] The *square root* of the number of independent constituents, which we have supposed to be twenty-five.

[2] See *Journal of the Royal Statistical Society*, "Mathematical Representation of Statistics," section iii., 1917, p. 65 *et passim*.

normal shape would persist, in spite of several modifications assimilating the case to that of price-variations. Then the causes tending to variation need not be perfectly independent. The steps need not be equal. There need not be several changes in each of the short periods. A variation may persist in one direction for several periods, provided that there is a chance of its being reversed. If, indeed, the last-named condition is removed on a large scale, the sub-normal character must disappear. Whether this or other abnormalities occur on such a scale as to vitiate the result is not to be decided off-hand, but by a careful scrutiny of the given statistics.

We have performed this scrutiny with respect to a set of 145 commodities selected by Professor Mitchell from the 241 above mentioned. We have examined the percentages presented by comparing the prices of 1913 with those of 1912 according to the "chain" method, and the percentages for 1913 according to the "fixed base" method with base 1890–99; and we are satisfied that the latter conform to the normal law at least as well as the former.[1]

But while thus questioning one of Professor Mitchell's premisses, we do not dispute his conclusion: "The longer a fixed-base system is maintained the more scattered become the relative prices as a rule" (37). Our discussion, however, warns us to accept with caution the corollary attached to this conclusion. "With a given body of quotations to build upon, chain relatives are more trustworthy than their rivals" (*loc. cit.*). Chain relatives relating to the preceding year are no doubt more trustworthy than their rivals when related to a much earlier period. But so are fixed-base index-numbers more trustworthy than a chain system if the former relate to the preceding year, the latter to a much earlier period.

The latter relation is dismissed too hastily in our judgment by Professor Mitchell. "Chain relatives for successive years . . . multiplied together to form a continuous series" (38) surely bring the later years into a relation with the earlier, which is as valid as most of the conceptions involved in an index-number?[2] We have performed this operation with respect to the chain index given by Professor Mitchell extending from 1890 to 1913; and have compared the result for 1913 with that which is given by

[1] The probable error for the group formed by comparison with a distant period, viz. nearly 20 (years), is to the probable error for the group formed by comparison between two consecutive years approximately in the ratio supposed in our illustration.

[2] Cp. *British Association Memorandum*, 1887, above, p. 219

the fixed-base method with base 1890–99. The difference appears at first sight marked; the median and the arithmetic mean according to the chain system being (for 1913) respectively 111·5 and 113, while, according to the fixed-base system, they are about 126 and 130 respectively. But it must be remembered that the chain starts from a greater height than the base of the fixed system; the level of 1890 (the base of the chain) being, with respect to the base of the fixed system, 113 or 114. This being taken into account, the consilience between the two systems is remarkable; and, indeed, greater than was to be expected.

It is tenable, we submit, that for certain purposes the chain system gives just as good a measure of the change in the price-level as the fixed-base system. For this reason we agree with Professor Mitchell that " it is an excellent plan to make from the original quotations two series of index-numbers—one a chain index and the other a fixed-base series."

Under the head of " Numbers and Kinds of Commodities Included " Professor Mitchell adduces a discovery for which students of his *Gold Prices under the Green-back Standard* [reviewed in the Economic Journal, Vol. XVIII. (1908), p. 581] will be prepared. He has observed that the fluctuation in price from year to year is much greater for some kinds of commodities than for others (52 *et seq.*). Thus manufactured goods are steadier than raw materials. There are characteristic differences among the price fluctuations of the groups consisting of mineral products, forest products, animal products, and farm crops. Again, consumers' goods are steadier in price than producers' goods, the demand for the former being less influenced by vicissitudes in business conditions. Knowledge of this kind may be used to explain the discrepancies between different index-numbers which mix these classes of commodities in different proportions. Professor Mitchell bases on this observation a recommendation that the commodities utilised in the construction of index-numbers should be classified, not (or not only), as now, empirically, or with reference to practical interests, but (also) " upon causal lines, upon differences among the factors which determine prices, upon a principle of division which throws more light upon the workings of the complex system of prices."

In considering that system Professor Mitchell has thrown light upon the complex systems. For in the course of his observation he brings into view the interdependence or *correlation* between the prices of different commodities. There is a similarity between the price fluctuation of finished products and raw materials. This, however, is less than the similarity between the price

fluctuations of finished products made from different materials.[1] The latter similarities, we surmise, are due to common causes, such as business cycles or changes in wages. The alternations of prosperity and depression no doubt affect all, or at least very many prices; but some much more than others. Thus the prices of minerals fluctuate with the alternations of business cycles better than the prices of other raw materials—farm and forest or animal products. Throughout the system there are found to be subtle correlations between observations which *primâ facie* are apt to be regarded as independent.

There is here exposed a feature which no doubt would be presented by other groups of statistics could they be as carefully examined. Observations seemingly independent are in reality honey-combed with correlations. Accordingly, calculations of probabilities based on the assumption of independence are apt to be inaccurate. Mathematical statisticians are too fond of calculating the "probable error" of averages on this assumption. They evolve, often with much labour and skill, a formula involving $n$, the number of observations, usually in the form of the *square root* of $n$ as a factor of the denominator. They forget that commonly the given number $n$ exaggerates the independence of the observations; owing to the existence of correlations, such as in the case of prices Professor Mitchell has so well expressed.[2]

Under the head "Problems of Weighting," Professor Mitchell propounds three questions: "Should the weights be sums of money or physical quantities? Should the weights be changed from year to year, or should they be kept constant? Should the weights be adjusted to the importance of the commodities as such, or should there be taken into account also the importance of the commodities as representing certain types of price fluctuations?" (78).

As to the first question, physical quantities measured by some

[1] The fact is thus happily expressed by our author: "As babies from different families are more like one another than they are like their respective parents, so here the relative prices of cotton textiles, woollen textiles, steel tools, bread, and shoes differ far less among themselves than they differ severally from the relative prices of raw cotton, raw wool, pig-iron, wheat, and hides."

[2] The great Laplace was not free from this assumption when he proposed to calculate the population of a country from the ratio between the number of baptisms and the population in different districts; and estimated the probable error of the calculation without taking into account the difference which no doubt prevails in respect of vital statistics between the inhabitants of different districts [cp. *Journal of the Royal Statistical Society*, Vol. LXXX. (1917), p. 549]. Probably only random samples such as those on which Dr. Bowley operates are quite —or at least very nearly—free from the influence in question (see Bowley, Presidential Address to Section F of the British Association, 1906; and *Livelihood and Poverty*, 1915).

conventional standard as a ton or a gallon are evidently improper weights for relative prices, ratios of which the type is $p_r$, the price of an article in the $r$th year, divided by $p_0$ the price of the same article in the base year (or period). But the *value* of the article (at the base, or some other suitable time) may properly be taken as the weight.[1]

As to the change of weights in view of change in the relative importance of commodities, Professor Mitchell points out that the compiler must choose between two evils, inaccurate weights and ambiguous price measures (79, referring to 31). With reference to fixed-base index-numbers, he considers that "the least objectionable compromise is probably to revise the scheme of weights, say, once a decade, and to show the effect of this change by computing overlapping results for a few years with both the old and new weights."[2] He puts "chain index-numbers" in a different category, for a reason that we have above questioned,[3] "since such series do not profess to yield accurate comparisons except between successive years" (80).[4]

Nor are we disposed to accept without qualification his answer to the third question, which is based on the following axiom. "An accurate measure of change in the level of all wholesale prices is not obtained unless all of the different types of fluctuation [referred to above, p. 394] . . . are represented in accordance with the relative importance of the commodities belonging to each." Very deep questions of first principle are here involved. We submit that the concept of an index-number for prices lies somewhere between two extreme definitions. One is the money value of a perfectly definite set of articles; for instance, a provision for certain functionaries of so much bread, sugar, uniforms, etc., from year to year (or at the same time in different countries). The sum total thus presented hardly deserves to be called an *index-number* (a title which, we observe with satisfaction, Professor Mitchell does not bestow as lavishly as some writers have done). Contrasted with a compilation which is of the nature of a commercial account is a true index-number, as described by Dr. Bowley: a quantity which we cannot observe directly, but which influences others which we can so observe.[5] The *quæsitum* thus conceived is related to the given price variations much as a physical quantity under measurement is related to a set of obser-

[1] *Cp.* below, par. 3.

[2] In illustration of this practice Professor Mitchell refers to Knibbs' *Cost of Living in Australia*, Commonwealth. . . Bureau . . . of Statistics Report, No. I., pp. xxiv, xlix.

[3] Above, p. 393.

[4] See *Elements of Statistics*, s.v. *Index number.*

[5] *Elements of Statistics*, chap. ix.

vations each purporting to represent the sought quantity. But the theory of errors-of-observation shows that in the combination of the given observations "less weight should be attached to observations belonging to a class which are subject to a wider deviation from the mean. Such would be prices of articles which, exclusive of the common price movement of all the selected articles, are liable to peculiarly large *proper* fluctuations." [1]

Ordinarily the required index-number is intermediate between the two extreme types which we have indicated. For even where the form is *primâ facie* a weighted sum—an aggregate of products each formed by multiplying a price by a quantity—still in our ignorance of the true factors the compound may assume the character of a true index-number.[2] Accordingly, a distribution of weight different from that which Professor Mitchell appears to prescribe would be ideally advisable. But where both the end to which our problem is directed and the means conducing thereto are so obscure and uncertain, we may acquiesce in our author's comment: "Perhaps it is a counsel of perfection to urge such requirements in systems of weighting."

It remains to consider the questions raised under the head "Averages and Aggregates." To some extent the answer to the questions under this and the preceding head will have been anticipated by the earlier discussion. For some decisions as to the scope and purpose of the index-number involve the choice of the method. Thus, if the purpose is that of Professor Irving Fisher, it follows at once that the proper combination of the data is the sum of the *values* (quantity × price) of the different commodities, each value weighted by the "turnover" of the commodity. The form of the index-number which purports to measure change in the cost of living is likewise predetermined. Ideally, at least, the form of a weighted sum is prescribed, though in practice it might be necessary to substitute some other.[3] The choice of average is wider in the case of other objects, of which some have been above mentioned.[4]

Professor Mitchell compares very fairly and fully the several available averages. Three stand out as selected candidates: the geometric mean, the median, and the arithmetic mean. With

[1] *British Association Memorandum,* 1887, p. 36. Cp. *Third Memorandum,* 1889, p. 157: "If more weight attaches to a change of price in one article rather than another it is not on account of the importance of that article to the consumer or the shopkeeper, but on account of its importance to the calculators of probabilities as affording an observation which is peculiarly likely to be correct."

[2] On this and other points connected with this discussion it may be allowable to refer to the present writer's *Lecture on Currency and Finance in Time of War* Clarendon Press), 1917

[3] Above, par. 2.

[4] Above, p. 386 *et seq.*

regard to the geometric mean, Professor Mitchell points out—in addition to other considerations in favour of this form—that it is dictated upon a certain conception of the purpose in view. "If that purpose be to measure the *average ratio of change* in prices, the geometric mean is the best; indeed, in strictness, it is the only proper average to employ." [1] But, continues our author, "as a rule our interest does centre in the money cost of goods rather than in the average ratio of changes in price."

If the geometric mean is ruled out, it remains to weigh the rival claims of the median and the arithmetic mean. Professor Mitchell strikes the balance more impartially than the majority of practical statisticians. Still, we think that even he has not done full justice to the median. Its defects in respect of convenience and accuracy appear slightly exaggerated in his presentation.

An objection of the first kind is thus stated: "Medians of different groups cannot be combined, averaged, or otherwise manipulated with ease as can arithmetic means." For instance, the Bureau of Labour Statistics, after obtaining the sums of relative prices for farm products, clothing, etc., can obtain by simply summing up these sums the grand average for all commodities. But "it could not handle medians in this convenient fashion; instead of combining the sums from the groups, it would have to combine the single commodities." This objection is true and serious. But it is not in practice quite so serious as it seems in statement. In order to obtain the median of a composite group, one compounded of two groups for each of which the median has been found, it is not in general necessary to "combine the single commodities" in the sense of re-examining them all. It suffices to re-examine and rearrange those which are in the neighbourhood of the respective centres.

For example, here are two groups each consisting of *twenty-seven* observations ranged in the order of magnitude, which observations have each been obtained by adding together ten digits taken at random from mathematical tables.[2]

| | |
|---|---|
| A. | 27 30 31 32 33 34 36 37 40 41 42 46 47 48 49 50 51 52 59 64 |
| | 30 41 42 46 52 |
| | 30 |
| B. | 29 31 32 33 34 36 38 39 41 43 44 45 46 49 50 52 53 56 57 59 62 |
| | 38 45 46 |
| | 38 45 |
| | 38 |

[1] Jevons probably meant something like this by his somewhat obscure dicta as to the grounds for preferring the geometric mean. *Investigations*, pp. 23, 121.

[2] These figures are adduced with some comments relative to the present subject in the *British Association Memorandum* for 1889, p. 59. In the third group there given one of the sums, above 50, has been omitted by a misprint.

The median, being the *fourteenth* observation in the order of magnitude, is, for Group A, 42, and for Group B, 44. To find the median for the group compounded of these two we need not re-examine all the observations. For it is evident that the median of the compound cannot be greater than the larger of the two medians, viz., 44; nor less than the smaller, viz., 42. Accordingly, we may thus summarily, for the purpose in hand, re-write the data.

| | | | |
|---|---|---|---|
| A. | XIII | 42<br>42 | XII |
| B. | XII | 43 | XIV |

Here the Roman numerals denote the number of observations which occur respectively above or below those given in Arabic figures. As the median of the composite group comprising *fifty-four* observations is intermediate between the twenty-seventh and twenty-eighth observations, it is evident at a glance that the required median is intermediate between 43 and 42, say 42·5. The process is easily extended to three or more groups.

However, we do not deny that the arithmetic mean has a considerable advantage over the median in virtue of the proposition, true of the former but not of the latter, that the mean of two (or more) means is the mean of the group formed by the constituents of both (or all) the several means.

Our difference with Professor Mitchell on another ground is more serious. He finds a difficulty in the use of the median in two opposite cases: when the given observations are either closely crowded, or widely dispersed about the centre of the group. As to the first case, it is said that the median may not answer precisely to its definition when several of the items to be averaged have identical values. For example, in Table II. [tabulating deviations presented by "chain" index-numbers] it often happens that the median falls on a large group of precisely identical figures, so that it ceases to be true that half of the cases are above and half below the median. Upon this it may be sufficient to say for the present [1] that the case in which there is an abnormal agglomeration about the centre is *primâ facie* one particularly favourable for the use of the median; since its probable error is less the greater, *ceteris paribus*, the height of the frequency curve at the middle.[2]

The opposite case of observations widely dispersed in the

[1] *Cp.* below, p. 404.

[2] More exactly, inversely proportional to the square root of the ordinate at the point on the abscissa where the median occurs. Cp. *Encyclopædia Britannica*, Art. "Probability," sect. 138, 139.

neighbourhood of the centre would be open to objection if the cause of the phenomenon were a depression in the form of the grouping, if the shape of the frequency curve about the centre resemble a valley between two eminences.[1] In that case, for the reason just now given, the probable error incident to the median would be particularly large. But this is clearly not the case contemplated by Professor Mitchell. He attributes the objectionable dispersion merely to the paucity of the observations. We shall therefore do no injustice to his argument if we suppose the grouping to be of an ordinary kind, in particular the normal law.

Upon this assumption it is at once to be admitted that the median is less accurate than the arithmetic mean, in so far as its probable error is a little greater, namely, in the ratio of about $1\frac{1}{4}$ to 1. That is all that we admit on the score of inaccuracy against the median. But we are by no means certain that we have apprehended Professor Mitchell's objections. Without being quite sure that we have located our author's position, we shall aim at three tolerably distinct points. (*a*) When there is a considerable interval between the position of the observation which forms the median and each of its nearest neighbours, then throughout a wide tract the position of the mean depends upon the accidents affecting a single observation. (*b*) The position assigned by the median is not perfectly definite. (*c*) The median is less responsive than the arithmetic mean to changes in the items.

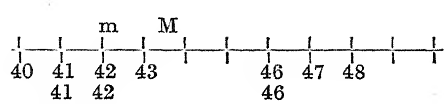
The first objection (*a*) is to be gathered from the following statements. "Where the numbers of items to be averaged is small, medians are erratic in their behaviour. . . . For in such groups there is often a considerable interval between the midmost relative price and the relative price standing next above it and next below. No change in any of the items, large or small, can alter the position of the median unless it shifts an item from the upper half of the list to the lower half or vice versa. But any change of this character, large or small, will make the median jump over the whole interval between its former position and that of the next highest or next lowest relative price, unless the change happens to place a new item within these limits" (85). Compare the dictum in the author's *Gold Prices . . . and Green-back Standard.* "The median . . . is rather erratic within limits of several points because its precise position is often dependent on the relative price of a single commodity which

[1] The supposition rejected as not appropriate to the data; above, p. 391.

stands in the middle of the scale of relative prices." [1] So again it is said: "When the numbers of commodities in the index-number is small, medians are likely to prove highly erratic, representing less the general trend of prices than the peculiarity of the data from which they are made" (90).

This objection is met by denying that the interval between two adjacent observations at the middle of the group is likely to be "considerable"; large relatively to the magnitude with which it is proper to compare that interval—that is, the *minimum mensurabile*, as we may say—that interval which is equal to (or of the same order as) the smallest degree which the compared method of measurement is capable of distinguishing with accuracy. For this minimum we may take at the least the "probable error" incident to the arithmetic mean. That the interval between adjacent observations is likely to be small compared with this minimum is sufficiently evidenced by the following proposition. When the number of observations ($n$) is large the interval at the middle of the group, which is as likely as not vacant, within which it is an even chance that no observation falls, is most probably very small compared with the probable range of the arithmetic mean (in the ratio of about $1 : \sqrt{n}$). When the number of observations is not large the proposition is less accurate. But it remains roughly true, as the number cannot be supposed very small consistent with the applicability of the theory of probabilities. Suppose, for instance, that the number of observations, is *twenty-five*, the number of a group which, according to Professor Mitchell, "illustrates the erratic character of the median." Then the space at the centre, which is as likely as not to be vacant, is about a quarter of the probable range to which the arithmetic mean is liable.

As a concrete example, let us take the groups above cited, formed by the addition of ten random digits. Here is reproduced the central region of Group A.:—

A.



The figures below the line represent observations which have occurred. The letter M above the line is meant to show the position of the arithmetic mean, being 43·6. The median, designated by the letter m, is coincident with one of the observa-

[1] *Op. cit.*, p. 58. Quoted with comment in the review of the work in the ECONOMIC JOURNAL for 1898, Vol. XVIII., p. 581.

tions, 42. There is here, no doubt, a vacant tract between 43 and 46. But it is not considerable, regard being had to the roughness of the computation. For the probable error to which the arithmetic mean is liable is nearly 1·2;[1] that of the median is about 1·5.[2] Had the median occurred anywhere between 43 and 46 there would have been no reason for suspicion.

M m

B. 39 41 43 44 45 46
45 46
45

Group B also does not countenance suspicion of the median. In its central tract here exhibited the largest gap is only 2; and the median, 44, as it happens, gives a better approximation than the arithmetic mean, 41·8, to the true value, which is 45.

If the object had been to ascertain whether there was any difference in the constitution of Groups A and B—whether B, for instance, had been constructed by the superposition of more or fewer digits than *ten* (the given number for A)—the median would give nearly as good an answer to this question as the arithmetic mean. The only difference is the one already acknowledged that the probable error of the median is a little larger. That difference would disappear if the number of observations (*in pari materia*) on which the median was based had been somewhat increased. The median of *forty* such observations would have afforded as good a test as the *arithmetic mean* of *twenty-seven*.

To give an example more germane to the present discussion, we have taken out, for the year 1890,[3] the relative prices that enter into the groups, numbering twenty-five each, which Professor Mitchell has instanced in connection with his remarks on the median. Here is exhibited the central portion of the first group or series. The arithmetic mean is at 115; the median at 115·5:—

M m

I. 108 112·5 115·5 118 119 121
119·3

There is a vacant gap of 3 in the immediate neighbourhood of the median and of 4 not far from it. But what of that, seeing

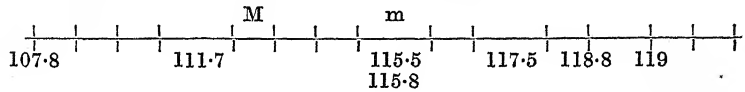
[1] $= 0{\cdot}4769 \times \sqrt{\dfrac{10 \times 16{\cdot}5}{27}}$ (nearly).

[2] $= \sqrt{\dfrac{\pi}{2}} \times 1{\cdot}2$ (nearly).

[3] From Table II. of the *Bureau of Labour Statistics*, 1914; whole number 149.

that the probable error of the arithmetic mean is about 3, and that of the median itself greater !

II.




The central tract of the second series likewise does not bear witness against the median. The arithmetic mean is at 113; the median is at 115·5; the probable error about the same as for the first series.

(*b*) There seems to be expressed a fresh objection in the statement that it " is not always true of medians " that " their meaning is perfectly definite " (91). The meaning of the objection does not seem to us perfectly definite. Possibly it belongs to the class of difficulties apprehended in the case of numerous observations. Perhaps it is the same as the objection which we have already mentioned under that head. Perhaps it is the same as an objection which has been levelled by other statisticians against the median, viz. that it does not in general present a value so finely graduated as does the arithmetic mean. Consider, for example, the " race " above imagined, run by tossing coins—say a dozen every five minutes—and taking a step—say a quarter of an inch—forward or backward, according as each coin turns up head or tail.[1] If a number of players each proceed thus—starting from a starting-point labelled 100 (25 inches from zero)—at the end of the period the group will be distributed *discontinuously* at integral points. Now the arithmetic mean is not limited to integers, it may occur anywhere between two adjacent integers. But *primâ facie* the median *is* so limited, or, rather, it seems to be limited either to an integer if the number of constituents be odd, or to an integer $+ \frac{1}{2}$ if the number of constituents is even. The objection is not particularly applicable to the data with which our author is dealing, relative prices graduated to a tenth of 1 per cent. In any case, the objection is not very serious, since by a proper adjustment of the data a fractional value can be obtained for the median.[2]

(*c*) There remains to be considered the objection that " arithmetic means are more representative averages than medians, being affected by any change in the items of the group " (85) . . . " unlike medians, they [" aggregates "] allow every change in the

[1] Above, p. 391.

[2] See the present writer's articles *On the Use of Analytical Geometry to Represent . . . Statistics*, and *On the Mathematical Representation of Statistics*. *Journal of the Royal Statistical Society*, Vol. LXXVII. p. 732, LXXIX. p. 471, *et passim*.

price of every article to influence the result" (71). In making this objection, the author seems to have in view two groups *in pari materia* such that in passing from one to the other we find no change in the median, while there are changes among the other observations other than those determining the median, which changes affect the more sensitive arithmetic mean. Upon this it may be remarked that if there is this difference in the behaviour of the two averages, it is not to the credit of the arithmetic mean. The slight advantage which we have already allowed to the arithmetic mean would not be enhanced by this circumstance. Supposing that slight advantage corrected by basing the median on a greater number of observations, then the sensitiveness attributed to the arithmetic mean would be rather a defect than an advantage.[1]

But does the difference exist? Does the median, oftener than the arithmetic mean, does it even often, remain unchanged from one group to another? This may be doubted, if the data are finely graduated, or if graduation of the median by adjustment is practised.[2] The median seems, indeed, but only seems, to be irresponsive in certain circumstances—of perhaps frequent occurrence in the statistics of prices—which we shall indicate by continuing the parable of the indoor race.[3] Suppose that in the first five minutes several of the numerous players—late or dilatory—do not make a start, and that their positions at the end of the period are registered as being at the starting-point. Accordingly, at the end of a short period a good number of observations would be heaped up at the starting-point; the median would appear unmoved. But, of course, the position of those players who have not moved—whose position is not the result of steps determined by tossing coins—cannot be used to ascertain the asymmetry of the coins. For *that* purpose it would be proper to omit those dead-head observations, or to prolong the game until the slow players should come in. But for *other* purposes, of perhaps greater interest to the players, as relevant to the betting, it might be proper to take account of those nullities.

[1] The arithmetic mean in this respect might be compared with the method of examination by summing arithmetic marks practised at some public competitions as contrasted with examinations at one at least of our Universities where general unanalysable impressions have a due weight. The former method, no doubt, more frequently brings out candidates as unequal, but the distinction does not correspond to a real difference.

[2] Above, p. 403, note 2.

[3] Above, p. 391. Note that the spike-shaped "mode" there noticed is formed by prices which have *not moved at all* in the period under consideration; to be distinguished from those which have moved less than one mill.

Here, probably, is to be found the reason of the difference between Professor Mitchell and ourselves as to the worth of the median. We have been all along seeking to extricate from fallible observations a mean apt to represent the "general trend of prices" (9). That is the sort of index-number to which we submit that the median may be appropriate. But Professor Mitchell in his criticism of this average has presumably often in view some of the more directly practical purposes which have been distinguished, such as *par excellence* the determination of changes in the cost of living. For these purposes we at once admit that the median is not so appropriate as the combination of the kind which Professor Mitchell calls an "aggregate." [1] We entirely agree with him that "the best form for general purpose series is a weighted aggregate of actual prices."

[1] The term "aggregate" is felicitous as suggesting approach to that type which, as above explained (p. 396), is furthest removed from an index-number, the term least connected with the Calculus of Probabilities; infelicitous so far as it masks the affinity, not to say identity, between the proposed construction and the weighted arithmetic means used by Giffen, Palgrave, and the older statisticians (as to whom see *British Association Memoranda*, 1887, p. 264, and 1889, p. 139 *et seq.*). The words of Sidgwick there quoted: "Summing up the amounts of money paid for the things consumed at the old and the new prices respectively . . ." (*Political Economy*, Book I. ch. ii. section iii.), are appropriate to aggregates.

## (O)

## EVALUATION OF THE METALLIC CURRENCY

[The attempts to evaluate the amount of coin circulating in a country which form the subject of this paper, published in the Economic Journal, 1891 and 1892, were conducted partly on the lines of Newmarch's method (discussed by the present writer at the meeting of the British Association for 1888), partly on the fresh lines struck out by Jevons. To the second class belong De Foville's calculation based on three French *enquêtes*, noticed here. Mr. F. C. Harrison's computation of the rupee circulation, which occupies a great part of the paper, is an improvement on the method of Jevons. By bringing to bear on the calculation the evidence afforded by the examination of samples pertaining to several successive years, he has obtained a result which seems to have almost the certainty of physical science.

The reader may like a reference to the *Journal of the Royal Statistical Society* (Vol. LXXXIII, 1920, p. 609 *et seq.*), where the subject is further discussed in connection with Mr. Shirras' excellent Paper on the effects of the war on gold and silver.]

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I. Among recent attempts to evaluate the amount of coins circulating in the country a prominent place is due to that which Messrs. Martin and Palgrave have just completed. Their method is similar to that which Newmarch employed to determine the circulation at the epoch 1843–4 (*History of Prices,* vol. vi.). They reason: As the percentage which the pre-Victorian sovereigns formed of the total circulation (previous to the recall of that coin) is to 100, so is the amount of pre-Victorian sovereigns to the total amount of sovereigns in circulation (previous to the recall); and similarly for the half-sovereigns. By means of circulars issued to bankers, Messrs. Martin and Palgrave ascertained that the percentage of pre-Victorian sovereigns was about 4 per cent.; and the number recalled was 2,335,000 nearly. Whence the total of sovereigns

previous to the recall is found to be about 58,375,000. Performing a similar computation for the half-sovereigns, deducting the coin recalled, and making an addition of £11,000,000 on account of the gold coin in the Bank of England which does not conform to the general average, Messrs. Martin and Palgrave (in their latest version, *Economist*, January 23) give £80,000,000 as the amount of the gold circulation.

Of the two data on which the inference mainly rests—the comparative and the absolute amount of the pre-Victorian coin —the former is corroborated, in the case of the sovereigns, by the close proximity between the observations for England and Wales, Scotland, and Ireland; 4·12, 4·1, 4·7 being the respective percentages formed by the pre-Victorian coin.[1] This consilience is not presented by the half-sovereigns, for which the respective percentages are ·84, ·50, and 1·06. But it may be observed that the numbers on which the Irish and Scotch averages rest are very small. The second datum, the absolute quantity of the pre-Victorian coin recalled, is too little by the number of coins not given up—retained, it may be, as curiosities. Against this deficit Mr. Martin—in his letter to *The Times* of July 21, 1861, describing the method of calculation—puts the fact that some of the recalled pre-Victorian sovereigns "undoubtedly came from abroad." The total officially known to have come from abroad is £162,751.

Both the data have been subjected to severe criticism in recent numbers of the *Economist* (January 2, 16, 23, 30). The majority of the objections which have been made suggest that the result obtained errs in defect. This contention, if it is substantiated, will confer on the computation the important character of a *lower limit* to the amount of coinage in circulation; thus rendering the Martin-Palgrave method complementary to that of Jevons, which—in its simplest form at least, when unmixed with precarious calculations based on the export and import of coin [2]—affords a *higher* limit. The two methods, if performed jointly, would give two limits between which the quantity of the coinage at the epoch to which the returns relate must lie.

II. Next may be noticed the brilliant attempt to estimate the rupee circulation which has been made by Mr. F. C. Harrison in the ECONOMIC JOURNAL.[3] His method is that of Jevons as to its

[1] This impression is confirmed by a more detailed inspection of the returns. The English sovereigns which were examined fall into four large classes, for which the percentages (of pre-Victorian coin) are respectively 4·2, 3·8, 3·5, 4·6.

[2] See Jevons, *Currency and Finance*, pp. 266–7.

[3] 1891 and 1892.

essence, but with a specific difference; the foundation is the same, but Mr. Harrison's construction rests, so to speak, on a great number of props, and they support each other archwise. Jevons, seeking to determine the amount of the (sovereign) circulation in 1867, reasoned: As the percentage (ascertained by the inspection of samples) which the coinage of 1863–4 forms of the total circulation is to 100, so is the amount of the coinage of 1863–4 presumed to be in circulation to the total circulation. Mr. Harrison, seeking to determine the amount of the (rupee) circulation in 1890, utilises similarly, not only the amount of the coinage presumed to be in circulation, but also the corresponding data for preceding years, *allowance being made for the greater diminution of the coinage of earlier years*. How is the comparative degree of diminution ascertained? By observing the gradually diminishing proportion which the coinage of any year, say 1874, forms in the circulation of successive years, 1877,[1] 1878, 1879–1890. These proportions are respectively: 2·13, 1·8, 1·6, 1·55, 1·45, 1·4, 1·3, 1·15, 1·2, ·95, ·9, ·95, ·9, ·9 per cent. of the total circulation in 1890. They measure the decrease of the coinage of 1874, upon the hypothesis that the total circulation is stationary during the period 1877–90; which Mr. Harrison assumes as approximately true (*op. cit.* p. 722). How is this assumption justified? By the consistency of the various results obtained on this hypothesis, a consistency which cannot be ascribed to accident. To show this, let us suppose that the decrease indicated by the row of figures above cited is due, not to the diminution of the amount of the 1874 coinage in the circulation, but to the increase of the total circulation with respect to which the percentages are taken. Upon this supposition the whole coinage of 1874 has passed into the circulation of 1890. But that coinage amounted to 4·352 crores[2] of rupees (as shown in Mr. Harrison's Table A); and it forms ·9 per cent. of the 1890 circulation (*ibid.*). Therefore (by Jevons's method) the circulation of 1890 = 4·352 ÷ ·009 or 483 crores, a result which is violently inconsistent, not only with all Mr. Harrison's estimates, but also with common sense, since the whole amount of the coinage issued *ab initio* is only about 300 crores (Table F).

It may be suspected, however, that the downward slope of

[1] Assuming with Mr. Harrison that the circulation of 1874 was three years in passing into circulation; and, after him also, at first leaving out of account the loss suffered by that coinage during those three years (*op. cit.* p. 733, and below, p. 411).

[2] It may be well to remind the reader that a crore = 100 lakhs = 10,000,000 rupees. Thus 4·352 crores = 43,520,000 rupees.

the percentages in question (the row of figures on p. 408), is due only partly to a real decrease in the coinage of 1874, and partly to the increase of the total circulation. But it will appear, I think, that the absolute constancy of the circulation during the period under consideration, 1877–1890, is of all simple hypotheses the one which best squares with the observations. For if the circulation is not constant, let it be allowable to suppose that it increases regularly, say, is multiplied by a factor $x$ which is greater than unity. Also let us suppose (in conformity with the data expressed in Mr. Harrison's Table D) that the whole of the 1874 coinage has passed into circulation by 1877; and that the *apparent* yearly decrease of that coinage—that is, the decrease of the percentage which that coinage forms in the total circulation—is 6·136.[1] Then the *real* yearly decrease of the 1874 coinage is given by the factor $x \times (100 - 6{\cdot}136) \div 100$, or $x \times {\cdot}93864$. Therefore the amount of 1874 coinage in the circulation of 1890 (thirteen years after the initial time, 1877) is $x^{13} \times ({\cdot}93864)^{13} \times 4{\cdot}352$ crores. But the proportion of 1874 coinage in the 1890 circulation is ·9 per cent. (Table A). Accordingly, the coinage of 1890 equals

$$x^{13} \times ({\cdot}93864)^{13} \times 4{\cdot}352 \times 100 \div {\cdot}9.$$

Or, taking logarithms,

$$\text{log. circulation of 1890} = 13 \log. x + 2{\cdot}3269....$$

By considering the coinage of 1875 I find another equation of the same form, namely—

$$\text{log. circulation of 1890} = 13 \log. x + 1{\cdot}9996....$$

The coinage of 1876 supplies another equation, and so on up to a recent year,[2] namely, 1886; the equation corresponding to which proves to be

$$\text{log. circulation of 1890} = 2 \log. x + 2{\cdot}0982.$$

Here, then, are *twelve*[3] simple equations involving two unknown quantities, the logarithm of the circulation of 1890 and the logarithm of the yearly increase $x$. Proceeding according

[1] This figure is thus obtained from the series cited on p. 408. The difference between the first and second of those figures (2·13 and 1·8) is 15·493 per cent. of the first; the difference between the second and third is 11·112 per cent. of the second, and so on. And 6·136 is the average of the percentages 15·493, 11·112, etc.—the *arithmetic* mean (not the geometric, which I should have preferred).

[2] In conformity with Table D, *op. cit.*

[3] Following Mr. Harrison, I do not utilise the returns for 1881 on account of the smallness of the coinage in that year, and the consequent irregularity of its incidents. No great difference, however, would be caused by including these data.

to the received rules prescribed by the Calculus of Probabilities for such cases, I find, as the most probable value of the circulation, 125 (crores); and as the most probable value of the factor constituting the yearly increase ·995, approximately unity. This result is confirmed by separately considering the first six and the second six of the twelve equations. From the first batch I find the factor 1·029; from the second batch 1·018. This calculation appears to me to have quite the rigour of physical science.

It will be noticed that the value for the circulation which has been obtained has the worth which attaches to the mean of a great number of observations? We may obtain a mean of equal worth more simply, once it has been ascertained that the currency is stationary, by calculating the circulation for 1890 from the coinage of each year separately and averaging these results. For instance:—Circulation of 1890 = Coinage of 1876 $\times$ $(\cdot 9486)^{13}$ $\times$ 100 $\div$ 1·6; where ·9486 is the factor whereby the coinage of 1876 yearly shrinks (Table D), 13 is the number of years elapsing from the year in which the 1876 coinage passes fully into circulation (*ibid.*), and 1·6 per cent. is the proportion of the 1876 coinage in the 1890 circulation (Table A). The result, which differs but slightly, and in virtue of a minute technical point,[1] from the result which Mr. Harrison obtains by a parity of reason in his Table J, is 129 crores.[2] Similarly calculating the circulation from the datum for each year except 1881 from 1874 to 1886 and 1874, 1878, 1886, and taking the mean of all the twelve results, I find 133 [3] for the circulation.

Or, again, we might have collected into a focus the single rays afforded by each annual observation, after this fashion: Determine the amount to which each coinage must have shrunk by 1890; add these amounts, and put the sum of them $\times$ 100 $\div$ the percentage which the 1874–1886 coinage forms in the 1890

[1] The point is that, in estimating the waste of the 1876 coinage during the eleven years, Mr. Harrison has worked with the decrements for each year, the percentages 6·250, 0, 6·6683 .... (Table D); whereas I have employed the *average* decrement 5·140. Mr. Harrison speaks of his principal Table as the bed of Procrustes in which all the coinages have been stretched. In performing this operation he has, so to speak, made each joint of the stretched victim to correspond to a particular part of the bed; whereas I have been content with a coincidence upon the whole—a procedure in favour of which there is not only classical authority, but mathematical convenience. It may be remarked that the difference between us would have disappeared, if I had employed (as I would, if he had given) *geometrical*, not arithmetical means of the yearly decrements of the coinage.

[2] See his account of this Table at the foot of his page 735.

[3] If the year 1881 is included, the result is 128 crores.

circulation (1874 and 1886 being the first and last years with which we operate) for the 1890 circulation. This method of averaging corresponds to Mr. Harrison's Table F.[1]

In thus proceeding we assume that each coinage has, if not for every year, at any rate on an average of years, its own rate of waste. But, if we assigned to all the coinages a common average rate of waste, we should come on the conception suggested, but not, I think, very happily worked out, by Mr. Harrison, in his Table E.

So far I have made abstraction of the second approximation, which Mr. Harrison performs by taking account of the waste suffered by each coinage before passing fully into circulation. Upon a probable hypothesis with respect to this waste, he is able to knock off some 10 per cent. from his results; and exhibits in his Tables G and K a new series of estimates smaller than the former, but still consistent with each other.

While we admire the marvellous convergence between different methods, we must not forget what it is they agree in establishing: namely, that a figure somewhere about 120 crores is not the amount of the rupee circulation—but a *superior limit* thereto. If each coinage, while passing into circulation, were to be diminished to any extent in one and the same ratio, multiplied by a common fractional factor $y$, we should have no means of detecting $y$. The whole beauty of the computation would survive, though much of its use would disappear. It is as if the arch, while remaining erect, with all its mutually supporting parts compact, should sink down as a whole owing to the treacherous softness of the ground. But the architect has secured his structure by certain external buttresses—let us hope incident on firmer ground—in the shape of independent estimates of the loss suffered by the coinages through export, hoarding, accident, and melting (*op. cit.* p. 739). It is remarkable that this collateral estimate—unlike the corresponding second approximation in the hands of Jevons—points to the conclusion that the primary estimate was *under*-estimated.

III. The only computation which can be compared with Mr. Harrison's in statistical interest is that which M. de Foville has founded on the monetary *enquêtes* which were conducted in 1878, 1885, 1891.[2] The French statistics are in some respects more imposing than the Indian, extending back over a much

[1] With, as before, a trifling difference.

[2] See *Bulletin de Statistique* for Oct. 1878, Aug. 1885, and Aug. 1891; also *Journal de la Société de Statistique*, Feb. 1879, Jan. 1886, and Nov. 1891.

greater number of years. The number of *coinages* figuring in the computation is much greater; but the number of *circulations* analysed is much less. Against the ten or twelve analyses of Indian circulation—the *columns* in Mr. Harrison's Table A—there are only three French monetary censuses. The web of the French texture, so to speak, is longer and more beautiful; but owing to the deficiency of warp the stuff has not equal consistency. The contrast thus indicated—the perfection of the French statistics in some senses, but their comparative weakness in that direction in which the strain of the reasoning is felt—may be illustrated by the following tables, relating to 20-franc gold pieces issued from the French Mint. The figures in the first row of Table I. are obtained from the figures (given in the *Bulletin de Statistique* for August, 1891, p. 147) which express the proportion between the number of coins of a certain date found at the *enquête* and the number of coins minted at that date; each of these figures has been divided by a certain fraction, viz., the total number of samples at the *enquête* ÷ total number of coins issued up to the date of the *enquête* (so as to reduce the returns for different *enquêtes* to a common denominator).

*Table I. showing the extent to which the coinage of particular years survives in comparison with the average survival of the coinage as a whole; as ascertained from the enquêtes of* 1891, 1885, *and* 1878 *respectively.*

| *Enquêtes.* | 1854. | 1855. | 1856. | 1857. | 1858. | 1859. | 1860. | 1861. | 1862. |
|---|---|---|---|---|---|---|---|---|---|
| 1891 | 1·1 | 1·1 | 1·0 | 1·2 | 1·1 | 1·1 | 1·1 | 1·1 | 1·3 |
| 1885 | 1·1 | 1·1 | 1·1 | 1·2 | 1·2 | 1·1 | 1·1 | 1·0 | 1·1 |
| 1878 | 1·0 | 1·1 | 0·9 | 1·1 | 1·03 | 1·1 | 1·1 | 1·1 | 1·2 |

*Table II. showing for decades what Table I. showed for single years; as ascertained from the enquêtes of* 1891 *and* 1878.

| *Enquêtes.* | 1803—12. | 1813—22. | 1823—32. | 1833—42. | 1843—52. | 1853—62. | 1863—74.[1] | Remaining period.[2] |
|---|---|---|---|---|---|---|---|---|
| 1891 | 0·4 | 0·4 | 0·5 | 0·6 | 1·6 | 1·1 | 1·1 | 0·9 |
| 1878 | 0·4 | 0·4 | 0·6 | 0·7 | 1·5 | 1·0 | 1·2 | 1·0 |

[1] There are no returns for 1872 and 1873.

[2] The remaining periods comprehend 1875, 1876, 1877, and 1878, in the case of the 1878 *enquête*; and, in the case of the 1891 *enquête*, the remaining years up to 1891, including 1891, and excluding 1880–85, during which there was no coinage.

These statistics of the survival of coins are certainly most perfect in their coincidence, probably far more regular than any vital statistics concerned with the ages of man—especially woman.

Yet for the particular purpose now before us it may appear that the French statistics are not so perfect as the Indian. M. de Foville's computation seems to occupy an intermediate position between the simple Jevonian and the highly compound Jevonian, or Harrisonian method. The beautifully regular figures which we have looked at are not those from which an approximate value of the circulation is directly found; the data on which the Jevonian method is best rested are figures formed like those in Tables I. and II., but greater than unity. The annexed table shows such figures as ascertained from the only two

*Table III. showing the extent to which the coinage of certain biennial periods survives in comparison with the average survival of the coinage as a whole; as ascertained from the enquêtes of* 1878 *and* 1891.

| *Enquêtes.* | 1875—76. | 1877—78. | 1879—86. | 1887—88. | 1889—90. | 1890—91. |
|---|---|---|---|---|---|---|
| 1891 | 0·7 | 1 | 0·7 | 0·5 | 2·3 | 3 |
| 1878 | 1·2 | 1·3 | | | | |

*enquêtes* which are available for this purpose. (For the *enquête* of 1885, made after a cessation of coinage for five years, does not, I think, present this phenomenon of terminal rise.) A little attention will show that the figures in this table, especially the last or penultimate figure in each row, are what we want for the useful application of the Jevonian rule of three. In fact, the result of that method may be defined as the total coinage up to the date of the *enquête* with which we are concerned *divided by* that figure in the corresponding row of Table III. which we select as best to operate with.[1] I cannot think that the proper figure is clearly indicated.

Each figure (*e. g.* 2·3) in Table III. =

$$\frac{\text{number of samples bearing a certain date (1889–90)}}{\text{total number of samples observed at a certain } \textit{enquête} \text{ (1891)}}$$

$$\div \frac{\text{coinage of that date (1889–90)}}{\text{coinage of all dates up to that } \textit{enquête} \text{ (1803–91)}}.$$

[1] Thus the number of French 20-franc pieces issued, from the initial date (1803) up to the present (1891), amounts to 362,809,000; and accordingly the Jevonian estimate for the present circulation (of French 20-franc pieces in France), as based on the data for 1889–90, is that amount ÷ 2·3, or nearly 158,000,000.

The last denominator, the coinage of all dates, *divided by* the figure specified (2·3) is the Jevonian formula for the circulation as deduced from the coinage of the biennial period selected (1889–90).

M. de Foville in his computation based on the 1891 *enquête* (*L'Économiste Français,* Sept. 19, 1891) uses in effect, as I understand, the datum corresponding to 1889–90. But was it not at least equally proper to include the datum for 1891; in which case his result would have been increased by some thirty per cent.? Again, as the terminal figures for the *enquête* of 1878 are so much smaller than those for 1891, while the total coinage up to the date of the *enquête* is not materially different—that of 1891 being larger than that of 1878 by only about 2 per cent.—we must suppose the circulation (of French 20-franc pieces within France) in 1878 to have been larger than in 1891. That is not paradoxical, considering that there has been little influx to compensate the evaporation, not to say drainage, of thirteen years. There is here nothing improbable, yet nothing probative. One misses the consilience of results to which the Indian statistics have accustomed us.

I am aware, of course, that M. de Foville has otherwise obtained the probative force of consilience. In particular, the correspondence between his computations of the gold and silver circulation is very reassuring. He first estimates the stock of silver at about 2,500,000,000 francs,[1] of which 1,200,000,000 are 5-franc pieces in active circulation. From the latter figure he passes to the existence of gold to the amount of 2,700,000,000 francs in virtue of the remarkably constant proportion, 31 : 69, between the gold and silver circulation attested by the *enquêtes.* And this estimate—taking account of the coins "immobilised" in the Bank and other circumstances—exactly squares with the application of the Jevonian method to the data of 1889–90, in such wise as to confirm the estimate of the gold circulation at 4,000,000,000 francs. Where several such coincidences concur, it seems as improbable that the computation should fail, as that a party of men roped together should all fall into a crevasse. I only say that there does appear to be a chink in the data to which Jevons's method is applied.

IV. Doubts would be removed and conjecture would be merged in certainty, if we had but one more datum, the net efflux (or influx) of coin in recent times, if only the statistics of the export of money could be relied upon. M. Ottomar Haupt

[1] Nominal value.

indeed does not hesitate to work with those materials; and (in the London *Economist* for October 3, 1891, and January 16, 1892) obtains an estimate for the French silver currency and English gold currency by a method setting the monetary exports against the imports, like that which Newmarch employed to determine the currency in years subsequent to 1844 (*History of Prices*, vol. vi. p. 703). But there are many who think that these statistical materials are too unsound to give support to any inference. As pointed out by Dr. Soetbeer (*Materials*, Taussig's translation, p. 352), there is a total failure of consilience between the recorded imports of precious metal into England from France and exports from France to England and vice versa. Not even when an average over many years is taken does an appearance of regularity arise. And it may be added that, if the difference between the efflux from and influx into England be deduced from the English and French statistics respectively, the results are still found to be totally disparate. Messrs. Martin and Palgrave, in an important letter to the *Economist*, January 23, 1892, add instances which have come under their personal experience showing the worthlessness of the declared values of monetary export. "Proved unsoundness" is the qualification applied to these statistics by the Committee of the British Association on the data available for determining the use of precious metals in a country.[1]

In the absence of this desideratum, it is to be feared that the Jevonian method is calculated to afford at best a *higher limit* to the circulation. Hence the peculiar worth of the Martin-Palgrave method, if affording a *lower limit.*

[1] See Report of the British Association for 1888.

## (P)

# DEFENCE OF MR. HARRISON'S CALCULATION OF THE RUPEE CIRCULATION

[THIS article, published in the ECONOMIC JOURNAL, 1900, forms a supplement to O; defending Mr. Harrison's method against certain criticisms contained in the *Report from the Head Commissioner of Paper Currency, Calcutta, to the Secretary to the Government of India, Finance and Commerce Department.* (No. 146.) 1899.]

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This document deserves notice here on account of the criticism which the Head Commissioner has bestowed on Mr. F. C. Harrison's method of evaluating the rupee circulation in India.[1] Mr. Harrison may comfort himself with the reflection which one of the older moralists offers to a person suffering under detraction: consider that you, as you really are, are not blamed, it is an imaginary character with attributes not yours that is held up to condemnation. The misrepresentation of Mr. Harrison's system has been effected by presenting only a part of it. The Head Commissioner describes it as an adaptation of Jevons's method; which he illustrates happily enough. This description does not comply with the schoolmen's rule of defining by *genus* and *differentia;* what is most characteristic and distinctive is omitted. True, Mr. Harrison's method belongs, as we may say, to the genus Jevons: but it is a very peculiar species, a highly composite variety of that genus. Jevons computed the amount of the circulation from the percentage of the total circulation formed by the coins of a particular recent date, together with the absolute number of those coins in circulation as given by the statistics of the mint. Mr. Harrison has based analogous estimates not only on recent coinages, of which the amount in circulation may be supposed to be approximately given, but

[1] For Mr. Harrison's explanation of his method, see ECONOMIC JOURNAL, Vol. I. p. 721, and Vol. II. p. 256; also Vol. VI. p. 122; and for some appreciation of the work, Vol. II. p. 162, and Vol. VII. p. 644.

also on older coinages, of which the amount in circulation is calculated from the yearly rate of waste. The peculiar cogency which the consilience of diversified computations imparts to the results has not been noticed by the critic. He tests the strength of the rope by detaching a single strand and subjecting it separately to a severe strain.

The sort of certainty which the physicist obtains by averaging numerous independent observations, attaches more particularly to that part of Mr. Harrison's reasoning by which it is concluded that the rupee circulation remained approximately constant for several years after 1876. His conclusion as to the absolute amount of the circulation does not appear to rest on quite the same foundation. For, as pointed out in a former number of the ECONOMIC JOURNAL,[1] if each coinage soon after leaving the mint were docked of a certain proportion, *e.g.*, by emigration or hoarding (in excess of that which Mr. Harrison estimates to be lost during the two or three years which a coinage may take in getting into circulation), the beautiful consilience of the results would not be affected, provided that the proportion abstracted were the same for each coinage; the constancy of the circulation in the years after 1876 would still be manifested, though for the absolute amount we should have only a superior limit. To make certain of the absolute amount we require another datum, which Mr. Harrison obtains by tracing the history of the coinage after it has left the mint, estimating how much is hoarded or melted or sent abroad in specie. If we likened the prime argument with its mutually supporting parts to a magnificent arch, this supplementary datum performs the part of an external buttress which secures the arch against a certain subsidence to which it may be liable. But all the strength and beauty of this architecture is lost on the undiscerning eye of the official inspector; he sees nothing but the foundation stone which was laid by Jevons.

Having failed to appreciate the cogency of the premises, it is no wonder that our critic should be sceptical about the conclusions. But it may excite surprise that he should suppose his case to be strengthened by the following remark:—

"Mr. Harrison's general conclusion drawn in 1894 from the above rather divergent results was that the circulation had from 1876 to 1886 been approximately constant at 115 crores, that it then gradually expanded to 120 crores in 1889–90, and had risen to about 125 crores in 1892–93. It may be reasonably

[1] Vol. II. p. 166. Above, p. 411.

doubted whether, with a population increasing at the rate of one per cent. a year, and with the issue between the years 1876 and 1886 of upwards of 70 crores of new rupees, the circulation can have remained during eleven years even approximately constant."

But according to Mr. Harrison's estimate of the waste of the coinage, about 6·77 per cent. per annum, the loss on a circulation kept constantly at 115 crores ought to be about 11 × 6·77 crores, that is, upwards of 70 crores. What wonder then that the addition of upwards of 70 crores of new rupees per annum should be just sufficient to keep the volume of the circulation constant! The observation which was meant as an objection proves to be a verification.

It sometimes happens that an advocate opening a strong case does not urge every tittle of evidence, whether through mere inadvertence or exercising a discreet reserve. If under these circumstances the counsel on the other side insists on cross-questioning he is apt to elicit some point damaging to his own case. The Head Commissioner has put himself in the unpleasant position of that cross-questioning counsel in the passage above quoted, and also in the following :—

" Mr. Harrison's examination was made in nine cases in the year immediately following the year of coinage, in six cases in the second year, and in one case in the third year. It seems very unlikely that equal diffusion can have taken place within one or even two years."

It will be remembered by our readers that Mr. Harrison selects for the date at which the coinage of any particular year (*e.g.*, 1874) may be assumed to have made its full contribution to the circulation, that year in which the proportion of the particular (*e.g.*, the 1874) coinage constitutes a maximum percentage of the circulation (in the case instanced the year 1877). This procedure is countenanced by the probability that the circulation was constant during the years under consideration. Still, as above admitted, the main argument in its bearing on the absolute quantity—as distinguished from the constancy—of the circulation is open to the suspicion that a uniform proportion of each coinage might, without any warning given, have been withdrawn from the amount assumed to have entered into circulation. The probability of this uniform subtraction becomes less the greater the variety of the circumstances under which the different coinages entered the circulation. Accordingly some additional probability is imparted to the whole argument when it is pointed out that there is a certain diversity in the data on which the different estimates are based.

Random as are the shots of the hostile critic, it is not to be expected that they should always be so very wide of the mark as actually to hit his own side. They mostly hit nothing at all. Thus in the continuation of the passage last quoted it is remarked :—

"The results arrived at were certainly irregular, for they represented the circulation in successive years to be 98, 110, 113, 142, 110, 107, 104, 108, 118, 143, 137, 157, and 133 crores."

The impression of irregularity is here conveyed by quoting one particular set of results out of the numerous sets which Mr. Harrison has obtained by consequent computations. When Mr. Bowley, by a masterly application of the theory of probabilities, concluded that the average money wage in England had remained constant over a period of years, it would have been no refutation of a conclusion based upon that theory to point out that the wages in a particular trade for that series of years were not constant, but irregular.

The Head Commissioner has peculiar notions about the nature of an average when, referring to Mr. Harrison's estimate of the average yearly loss of coinage, he remarks :

"This very precise figure can hardly be admitted to be correct for all years."

As if an average could be expected to be equal to each of the items averaged !

A knowledge of the theory of averages removes the sting from the following remark :—

"Mr. Harrison is, apparently, himself dissatisfied with the outcome of his labours, for, when examined last year as a witness before the Currency Committee, he stated that he would not be surprised to find that the circulation, instead of being 128 crores (his latest estimate), was found to be 140 crores on the one side, or 90 crores on the other."

It is not quite clear what degree of surprise Mr. Harrison intends to indicate by this *obiter dictum.* A person who plays backgammon often would not be very much surprised at throwing three aces in succession. If Mr. Harrison is to be understood as assigning a corresponding degree of improbability to the limits 90 and 140 being overstepped, it follows from well-known principles that, supposing the usual law of error to prevail, at least roughly, a "probable error" of about 6 is attached by Mr. Harrison to his estimate of 115. Surely a result may be of some practical value even though liable to this degree of error. For instance, an index-number of prices may well be liable to as great a probable deviation.

Our confidence in this result is not much shaken by the existence of certain inaccuracies in the data :—

"The returns cannot be accepted with implicit confidence, for every now and then evidence appears of neglect of duty. Thus, in May 1894, two Bengal Treasuries reported the existence of coin dated 1894, though there is no such coinage, and it was afterwards acknowledged that the entries were mistakes. The same has happened in 1898, and again in the present year [1900] when the Amritsar Treasury returned 87 rupees of 1894, 100 of 1895, and 116 of 1896, while Sealkote reported 129 of 1894, 165 of 1895, and 116 of 1896, though there is no genuine coin of those dates. All the neighbouring treasuries found no rupees of those years, and it must be concluded not that there are perfectly made counterfeits in the Punjab bearing those dates, but that the persons entrusted with the duty of examining the coins have, to use a slang expression, *fudged* these returns."

The correction of these errors does not seem to affect sensibly the average result. The incident is paralleled by the French monetary statistics from which M. De Foville, according to the method of Jevons, has calculated the number of five-franc pieces in circulation. A certain number of offices, he tells us, made returns of coins as having dates at which there was no French coinage. But the errors were not on a scale to appreciably vitiate the calculation; the only consequence was to teach the offenders, by a severe reprimand, the danger of being too witty.

The Head Commissioner concludes by reflecting on the ingenious "inverse Jevonian" method which Mr. Harrison has based, not on the addition of new coinage to the circulation, but on the withdrawal of the coinage of date earlier than 1836. Taken by itself, Mr. Harrison admits, this elegant construction would not be strong enough to support the conclusion; if only for the reason that, as the amount of coin withdrawn forms a small percentage of the total circulation, a comparatively small error in the former is apt to considerably vitiate the latter. But, considered as an additional strand in the coil of circumstantial evidence, which is all that Mr. Harrison claims, this supplementary method has a certain cogency which the Head Commissioner's disparaging reflections do not invalidate.

On the whole, it appears to us that Mr. Harrison's method emerges from the test of the hostile criticism to which it has been subjected, not only unscathed, but even with added lustre.

*Merses profundo, pulchrior evenit.*

## (Q)

## QUESTIONS CONNECTED WITH BIMETALLISM

[THE "Thoughts on Monetary Reform" which form the subject of this article (ECONOMIC JOURNAL, 1895) were suggested by contemporary agitation in favour of Bimetallism. In spite of the presumption in favour of an argument on which Giffen and Professor Foxwell agreed, I dispute that there is "a permanent tendency" to appreciation of money, that "a fall in prices may be regarded as the normal condition of things." Some illustrations of the quantity theory, with special reference to what Giffen called the *dynamical* character of the problem, are next offered. The fall of prices, it is argued, is not an unmitigated evil, not an intolerable burden on debtors; on the supposition that the appreciation was due to abundance of commodities (relatively to money)—a supposition not relevant to present (1923) problems. Lastly, the variant form of bimetallism proposed by Marshall, here named "Symmetallism," is discussed at length. As compared with ordinary bimetallism it is found to have the advantage in almost every respect except that of familiarity.]

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The following is an attempt to estimate the force of some few out of the many considerations which bear upon proposed monetary reforms.

I. The argument first to be considered is one which is suggested by Mr. Giffen [1] in a passage quoted by Professor Foxwell in his evidence [2] before the Agricultural Commission.

In Mr. Giffen's words, "There is an intrinsic difficulty in the way of an increase of a standard metal used as money proportionate to the increase of the commodities which it moves. As the latter are renewed incessantly, an increase in the means of production increases the whole mass on the market at any given time. As the precious metals in use, however, exist in masses enormously

[1] *Essays in Finance*, Vol. II. p. 30. [2] Questions 23,646 and 23,647

greater than the whole annual production, an increase of the means of production equal to what takes place in other commodities only means, in the case of gold, an increase of a fraction of the whole mass in use. There is accordingly a permanent tendency to change in the relation of commodities to gold." As Professor Foxwell understands, the argument is that "a fall in prices may be regarded as the normal condition of things." [1]

An argument advanced by Mr. Giffen and entertained by Professor Foxwell is not likely to be open to dispute. It is with great diffidence that the following counter-reasoning is submitted.

An increase in the annual production of gold corresponding to the increase in the annual production of commodities implies the increase—not merely of a fraction, but—of the entire mass of gold. For consider that mass as made up of parts distinguished as (1) new, (2) one year old, (3) two years old, etc. And let us at first make abstraction of the circumstance that the mass is continually being diminished by loss and wear. Then in the lapse of a year part (1) by hypothesis is increased in the same proportion as the mass of commodities—a proportion which may be supposed uniform from year to year. Also, in the lapse of a year, while part (1) is freshly created, what was part (1) the year before becomes part (2) of the present year. By parity of reasoning, part (3) of the present or ultimate year is the same as part (2) of the penultimate year—that is, the same as part (1) of the antepenultimate year. Therefore, upon the hypothesis made, part (1) of the ultimate year is to part (1) of the penultimate year in the ratio which expresses the increase of commodities from year to year; part (2) of the ultimate year is to part (2) of the penultimate year as part (1) of the penultimate year is to part (1) of the antepenultimate—that is, in the aforesaid ratio of the commodities of one year to the commodities of the preceding year. By parity of reasoning, the same ratio is found to prevail between all the corresponding parts of the mass of precious metal in the ultimate and penultimate years; and therefore between the whole masses.

This reasoning is not materially affected when we restore the concrete circumstance, that the mass of precious metal is liable to waste. The argument may be made clearer by the subjoined mathematical analysis.

Let $G$ be the amount of gold existing at some remote period, such that the subsequent additions to the precious metal far exceed $G$. Let $g$ be the amount of gold added to the stock in the course of a year at the initial period. Let $W$ be the amount

[1] *Loc. cit.*

of wealth or wares existing at the beginning of the first year, produced during the preceding year. And let it be assumed that $W$ and $g$ increase from year to year in the same uniform ratio—viz. $1 : (1 + \rho)$. Let it further be assumed that all parts of the stock of gold are diminished by waste annually in the ratio $1 : (1 - \sigma)$, where $\sigma$ is a proper fraction.

Upon these suppositions the masses of goods at the end of the years 0, 1, 2, and $n$ are $W$, $W(1 + \rho)$, $W(1 + \rho)^2$, . . . $W(1 + \rho)^n$, respectively. The corresponding masses of gold are

$$G,\ G(1 - \sigma) + g,\ G(1 - \sigma)^2 + g(1 - \sigma) + g(1 + \rho),\ \ldots$$
$$G(1 - \sigma)^n + g(1 - \sigma)^{n-1} + g(1 - \sigma)^{n-2}(1 + \rho) + \ldots + g(1 + \rho)^{n-1}.$$

The first term of the general expression may be neglected in comparison with the remainder; by hypothesis, even upon the supposition that $\sigma$ were zero; *a fortiori*, if $\sigma$ is considerable as may be. The expression thus reduced may be put in the form

$$g\frac{(1 + \rho)^n - (1 - \sigma)^n}{(1 + \rho) - (1 - \sigma)}.$$

The mass of gold at the end of the $m$th year is found by substituting $m$ for $n$ in the above expression. Therefore the mass of the $m$th year, divided by the mass of the $n$th year, is

$$\frac{(1 + \rho)^m - (1 - \sigma)^m}{(1 + \rho)^n - (1 - \sigma)^n} = \frac{(1 + \rho)^m}{(1 + \rho)^n},$$

in the limit, when $m$ and $n$ are large. But $(1 + \rho)^m : (1 + \rho)^n$ is the ratio of the masses of commodities in the $m$th and $n$th years. Whence it appears that *a constant level of prices, rather than a fall of prices, is to be regarded as "the normal condition of things."*

Nor is it necessary that the rate of waste should be the same for the new and the old gold (or silver). For consider the fraction of which the numerator is the mass of gold in the $m$th year, the denominator the mass in the $n$th year (from an initial epoch); $m$ being greater than $n$. Let $\sigma$, $\sigma_2$, and $\sigma_n$ represent the percentage of waste suffered by a portion of the metal during the first, second . . . $n$th year of its existence. The diversity of these coefficients does not invalidate the proposition that to each term of the denominator—*e.g.*,

$$g(1 + \rho)^{n-3}(1 - \sigma_1)(1 - \sigma_2)$$

there corresponds a term of the numerator greater in the ratio of $(1 + \rho)^{m-n} : 1$, *e.g.* :—

$$g(1 + \rho)^{m-3}(1 - \sigma_1)(1 - \sigma_2).$$

Accordingly the whole numerator exceeds the whole denominator in that ratio. That is, assuming as before that in $n$ years the

waste is very great. But if this is not assumed, if the rate of waste—like that of cooling—slackens with time, then there may enure to the benefit of the numerator a substantial remainder, a very long *etcetera*. Accordingly *there is some probability that a rise in prices may be regarded as the normal condition of things*.

The argument continues to hold when the law of waste is supposed to be different for gold and silver applied to different uses, *e.g.*, money and the arts. Also there may be different classes of goods not all worn out in one year. The coefficient of waste for goods, say $s$, may be unity for corn, of which the yearly supply is consumed in the year but three-fourths for boots, agreeably to the supposition that by five years boots on an average will have been reduced to less than $\frac{1}{1000}$ part of their original value: $(1 - \frac{3}{4})^5 = \frac{1}{1024}$.

The argument breaks down only when $\rho$ is not constant, the production of commodities increasing from year to year at an increasing rate. But, as submitted in the next section, this case is not known to be a real one.

Of course it is possible, that while the stock of precious metal increases uniformly with commodities, other things are not uniform: in particular the proportion of precious metal used in the arts and as money, and the factors, described in the next paragraph, which intervene between the quantity of metallic money and the level of prices. And it is possible that the variations of these quantities should counteract each other. The reasoning is very abstract, but so is the reasoning against which it is directed.[1]

This argument, whatever its force, is in the direction of monetary reform. The reformer can no longer be represented as one who pretends to avert a decline which is inevitable—like the charlatan who pretends to have discovered the elixir of life, or, as recently announced, the "microbe of death."

II. If the recent fall of prices cannot be explained as "the normal condition of things," to what cause is it to be attributed? This inquiry forms my second head.

On this point I have little to add to what Mr. Giffen has said about the *dynamical* character of the problem.[2] We must con-

[1] Thus no account is here taken of the argument that the yield of mines, unlike the production of commodities in general, cannot be indefinitely multiplied. The abstraction of this consideration will appear allowable to those at least who hold that predictions about the production of gold in the distant future are, as Newmarch is reported to have said with reference to the statistics of gold-production, not worth the paper they are written on.

[2] *The Growth of Capital*

ceive the masses of both goods and metallic money as continually growing; and in order to account for a change in their relation it is not enough to show that one of the quantities has increased, for both of them are to be conceived as continually increasing.

We might figure the discrepancy between goods and gold which is indicated by a fall in prices as a change in the distance between two moving bodies.[1] It is generally easier to ascertain the fact that such a change has occurred than to assign the cause to one or other of the moving bodies. In an Oxford boat-race Corpus bumps Balliol. That is a plain fact. But it may be a fine issue whether the cause is the acceleration of Corpus, or the retardation of Balliol. It may happen, no doubt, that both cause and fact are equally manifest; as when Ajax Oileus, racing against Ulysses at the funeral games of Patroclus, slipped in the mire, and so was distanced by his rival. But the incidents are not so simple in the race which we have to contemplate. The Homeric chariot-race might afford nearer parallels. When, Diomed having dropped his whip, his rival began to gain upon him, the spectator would naturally connect the two events. So the change of monetary legislation in 1873 being apt to produce a fall of prices, and being followed by a fall of prices, is naturally assigned by some theorists as the cause of the fall. The circumstance which is assigned by other theorists as the cause of the fall—namely improvements in arts of production, attended with an acceleration in the growth of commodities—does not excite belief so readily. The Homeric parallel of this cause is: Pallas inspiring force into the horses of her favourite. The intelligent spectator might require proof that this agency only came into operation just before the distance between the rival chariots began to diminish. Before ascribing an access of spirit and vigour to the team which seemed to be gaining ground, he would require pretty accurate observations of its velocity before and after the alleged inspiration.

But of course it is a very elementary conception to regard prices as the quotient of the volume of goods divided by the mass of metallic money. When we take account of the complicated factors affecting the fall of prices, we shall be still more cautious about locating the cause. It is quite conceivable that neither the monetary legislation of 1873, nor the subsequent rapid augmentation of production, would by itself have sufficed to produce the observed effect. The concurrence of the two was required.

[1] The device of employing a distance to express a ratio is used by Cournot in a cognate inquiry. *Recherches Mathématiques,* ch. ii.

We may conceive prices as representing the relation between the *volume of goods* in a sense which is implicit in most definitions of an index-number, and the *mass of currency* in the sense of metallic money augmented by credit; account being taken both of the number of times each piece of goods is sold, and the number of times each piece of money or instrument of credit effects a purchase. The *volume of transactions*, and the *momentum of circulation*, thus conceived, are not to be regarded as growing like two independently moving masses, but rather like two bodies connected by a link which it requires some violence to shorten or elongate—such is the force of habit resisting a general change in prices. This relation may be preserved constant, in spite of a disparity between the quantity of goods and metallic money, by the accommodating elasticity of other factors, in particular the amount of credit. It is attempted in the annexed diagram to give a rough idea of the relations between the several growing quantities figured as bodies moving along a line.


FIG. 1.

Here W is the volume of goods, C is the mass of currency, V is the volume of transactions, Q the momentum of circulation, as above defined; in the region under consideration, say the system of countries having a gold standard. The distance QV represents a ratio measured by an ideal index-number—such a one as the present writer in his third memorandum on variations in the value of money (British Association Report for 1889) has connected with the name of Professor Foxwell.

Say Q and V were coincident at an initial epoch 1867–77, and that now the proportion of the quantity V to the quantity Q is, in round numbers, $1\frac{1}{2} : 1$, corresponding to an index-number 66·6. Then QV—about a quarter of an inch—stands for log 1·5. The distance VW represents the number of times that each piece of goods is sold on an average per unit of time, that is $(1{\cdot}5)^2$, or 2·25, if VW is about half an inch. The distance QC represents the number of times that each piece of money effects a purchase on an average per unit of time, according to the figure, also 2·25.

The quantities W and C are respectively connected, the former with the volume of transactions V, the latter with the momentum of circulation Q, each by a link which, under pressure or tension, is readily shortened or elongated telescope-wise. A similar, or even more yielding, bond connects C, the quantity of

currency, with G, the quantity of gold used as money. But V is yoked to Q more firmly, as it were, by a pole made of some viscous substance which does not easily alter its length. The thick line QV and the dotted line GC are intended to indicate different degrees in the rigidity of connection. The connections between V and W and between Q and C are perhaps of intermediate rigidity.

But really the mechanism of this complicated yoke is very imperfectly known. No one is competent to predict what will be the effect of any assigned stress or strain. (Upon the extent of our ignorance, compare Professor Marshall's evidence before the Precious Metal Commission, Q. 9,629.) It is possible that a slight spurring of W or curbing of C will produce, not a slight effect, but no effect on the length QV. One is free to indulge the following compromise hypothesis. The acceleration of W, due to an increased rapidity in the growth of goods since 1873, would not of itself have sufficed to elongate QV—to depress general prices. That tendency would have been counteracted by the elongation of GC or CQ, or the abbreviation of WV. Nor would the acceleration of W, due to an increase in the amount of goods to be moved by gold upon the demonetisation of silver, of itself have sufficed to alter CW. Both impulses—and perhaps at the same time a certain retardation of G—were necessary to produce the observed effect. Thus both parties in the controversy about the cause of the appreciation of gold may be right—both those who attribute it to the increase of goods, and those who attribute it to a monetary derangement.

It may be observed that this hypothesis, too, forms an argument in favour of monetary reform. For if the recent disturbance of prices would not have occurred without injudicious monetary legislation in the past, it is possible that judicious monetary legislation in the present may prevent future disturbances.

III. But is the fall of prices an unmitigated evil? I submit that a considerable mitigation is afforded by a circumstance which—however its importance as a cause may be disputed—is not denied to be a fact: namely the increased production of goods per head in the civilised world during the last twenty-five years.[1]

This circumstance tends to remove the grievance which con-

[1] It will be noticed that the argument in this section requires only an increase in the quantity of goods per head, not, like the argument in the former section, an *acceleration* in the growth of goods. What is required for the present argument appears to be fully proved by several writers who have supported the former argument: in particular Nasse, Wells, Atkinson, Pierson.

stitutes the strongest motive in favour of monetary reform—namely the pressure of fixed debts upon shrinking incomes. As Professor Foxwell explains in his masterly evidence before the Agricultural Commission (Q. 23,815), "Business is injured when the prices of commodities fall. . . . What depresses trade is the falling in commodities." The entrepreneur is embarrassed when he has to pay his creditors an increased quantity of commodities. But if the production of commodities per head has increased since the debts were contracted, then the average entrepreneur, though he may have to pay his creditors more commodities, will have more commodities out of which to make that payment.

In the words of Professor Taussig,[1] endorsed by the authority of Professor Lexis:[2] "The rise in money incomes and the improvements in production disprove any intolerable burden on debtors, and make it highly improbable that the change has had any general depressing effect on industry."

Where is the great hardship? What does the bimetallist complain of? (*loc. cit.* Q. 23,815).

"If you fix your money by incomes, and allow the prices of commodities to fall, it comes to this, that the creditor gets the whole advantage of the fall in the prices of commodities, to which as creditor he has contributed nothing."

The creditor does not get the whole advantage; the recipients of wages and of profits have their shares increased in proportion to the increase of industrial efficiency. Take the figures for the changes in the national income of the United Kingdom recently given by Mr. Bowley,[3] and compare the average of the years 1874 and (the next year in his tables) 1877 with that of 1886 and 1891 (his last years). Money income has remained almost constant during this period. The average national income for 1874 and 1877, compared with 1860, taken as 100, was $\frac{1}{2}(143 + 142) = 142{\cdot}5$; and in 1886 + 1891 exactly the same—viz., $\frac{1}{2}(138 + 147) = 142{\cdot}5$. Meanwhile the real average income had risen from $\frac{1}{2}(139 + 148)$, or $143{\cdot}5$, to $\frac{1}{2}(198 + 202) = 200$, that is about forty per cent. Also real average wages have increased from $\frac{1}{2}(134 + 139)$ to $\frac{1}{2}(180 + 192)$, that is about thirty-six per cent. Therefore real average income not received as wages must have increased by more than forty per cent. Consider this portion of income as made up partly of fixed payments—say interests and rent—and partly of profits. If all the fixed payments dated from 1874–77—an extreme supposition—

[1] *Silver Question*, Part II. p. 106. [2] *Jahrb. f. Nat. Oekon.*, March, 1894.
[3] *Journal of the Statistical Society*, June, 1895.

the real average income of the recipients would have increased by forty per cent. But, as just concluded, profits + fixed payments have increased by more than forty per cent. Profits then must have increased by more than forty per cent. Profits have had their full share of the increased national wealth, even on the extreme supposition which has been made. The creditor then does not "get the whole advantage." Nor is it true that "he has contributed nothing" to the growth of the national income. But for his saving it would have been smaller.[1]

A diagram may make the argument clearer. Let the gross income of an average or typical entrepreneur, measured in money, at different epochs, be represented by the parallel lines PS and ps, which are divided into proportional parts by four lines meeting in the point O.

Let PS represent the incomings before the appreciation of gold;

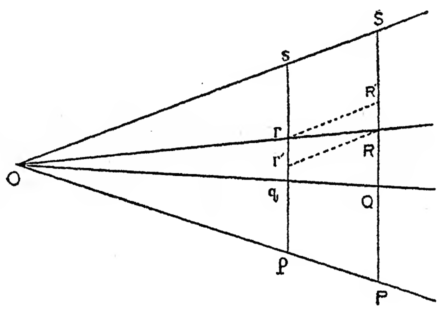


FIG. 2.

PQ, expenses which may be called current, such as outlay on material, which vary with the variation of prices; SR, fixed expenses, such as interest of debts or rents on long leases; the remainder, RQ, the profits of the entrepreneur. And let ps represent the incomings after the appreciation of gold. Then pq, the current expenses, will be diminished in the same proportion as the gross income. Thus PQ : pq : PS : ps. But the fixed expenses will not be thus diminished. The new fixed expenses are the same as the old, viz. sr′ (determined by drawing Rr′ parallel to Ss). Accordingly the profits of the entrepreneur are reduced by the burden of fixed debt from qr to qr′, which may become very small. Hence all these tears.

This reasoning takes for granted that the gross income of the entrepreneur measured in commodities, is the same at the two

[1] See Professor Marshall's weighty remarks on this subject in the discussion on Mr. Bowley's paper in the *Journal of the Statistical Society*, June, 1895.

epochs. But there is reason to believe that the total production per head, and therefore the production of the average entrepreneur, has increased in recent years in about the same proportion as gold has been appreciated.[1]

This case may be represented by the same figure differently interpreted. Let ps now represent the gross income of the typical entrepreneur, measured in commodities, at the initial epoch; and PS his gross income at a subsequent epoch, when labour, so to speak, and gold have both been appreciated with respect to commodities in the same ratio, viz. Op : OP. The shares into which the gross income is distributed are at the initial pq current expenses, sr fixed expenses, and qr profits (all measured in commodities); and at the subsequent epoch (the same measure being employed) PQ current expenses, SR fixed expenses (since the gold which was the equivalent of sr commodities has become the equivalent of SR), and accordingly QR profits.

According to the arrangement that the creditor should receive a constant quantity of commodities Sr′ (determined by drawing rR′ parallel to Ss), a special advantage, to the extent of RR′, is given to the entrepreneur who has debts of long standing, in comparison with one whose expenses are mainly of the current kind. According to the existing arrangement, if the assumptions which have been made as to the increased efficiency of labour are accurate, the borrower is, on an average, as well off as he who has not borrowed; if the assumptions are only slightly exaggerated, the borrower is not much worse off. Is this such a flagrant injustice? Is it one which it is worth making a very great effort to rectify?

Of course all this polemical reasoning is highly abstract; but so is the reasoning against which it is directed. The state of things which forms the second interpretation of our figure is a sort of representative mean from which concrete particulars diverge enormously—for instance the condition of the English agriculturist. But so is the state of things conceived by the bimetallist forming the first interpretation of the figure a mere abstract representation which is not applicable to whole classes of business—for instance the case put by Mr. Pierson (Economic Journal, Vol. V. p. 110), in which the prices of materials (our "current" expenses) fall sooner than the prices of the finished article (our "gross incomings"). The burden, which is characteristic of the general type, is not felt in this particular case. So the

[1] Consult, in addition to the authorities above cited, those referred to in the Economic Journal, Vol. IV. pp. 158—165.

general remedy would fail in the extensive case of German agriculture, if Professor Lexis is right in predicting that in this case bimetallism would inflate the expenses of production more than the prices of the product.[1]

IV. Let it be admitted, however, that a currency steadily expanding with commodities, or at any rate steady in some definite sense, is desirable. Let it further be admitted that steadiness is more likely to be secured by the use of two metals than of one only. But bimetallism is not the only method of combining the two metals. The form which bimetallism has most recently assumed presents an additional reason for believing that a more excellent way is that which Professor Marshall has proposed,[2] according to which the standard is not a unit of gold *or* so many units of silver, but a unit of gold *and* so many units of silver—a linked bar on which a paper currency may be based. The arrangement that there should be a *joint demand*[3] for gold and silver money might, perhaps, be called *symmetallism*, to distinguish it from the arrangement that there should be a *composite supply* which is called bimetallism.

The one advantage which bimetallism has hitherto enjoyed over its younger rival is historical precedent, familiarity, and the better fulfilment of Mill's condition for a sound currency, that "it should be intelligible to the most untaught capacity." But this one advantage appears to be impaired in the most recent form of bimetallism. For, whereas the definition of bimetallism used to be "an open mint ready to coin any quantity of gold and silver,"[4] we read now of purchasing gold and silver bullion with legal tender. In Professor Foxwell's clear and authoritative words:—

"According to the views which are very current now in bimetallic circles, the bimetallic mintage would mainly be a mintage consisting of a return of paper or certificates for bars" (Q. 24,334).

"The person tendering the bullion receives legal tender" (Q. 23,751).

"The debtor would pay exactly as he pays to-day, by cheques

[1] See p. 281 of the ECONOMIC JOURNAL, Vol. V. 1895. The reference is to an article by Professor Lexis in the ECONOMIC JOURNAL, 1905.

[2] Evidence before the Gold and Silver Commission (1888), Q. 9,837. Compare the proposal made in Dr. Hertzka's *Das Internationale Wahrungsproblem* (1892).

[3] See Marshall's *Principles of Economics*, Book V. ch. vi.

[4] The definition given by the Gold and Silver Commission (Final Report, Part I. par. 116) after Mr. Hucks Gibbs. So Professor Sidgwick in his *Political Economy*, Book III. ch. iv. § 6, speaks of the "plan known as bimetallism, *i.e.*, coining gold and silver freely and making them legal tender." (Cp. *ibid.* II. v. 6.)

and notes, or instruments of that kind which are based upon reserves in the banks; but the debtor would never consider what his note or cheque was based upon, whether upon silver or upon gold, any more than he does in France at the present day" (Q. 23,756).

Now all that is said so truly and persuasively here, and in the context, of bimetallism, is equally applicable to symmetallism. And the doubts and difficulties felt by the monometallist interrogators could not have been greater if the witness had been advocating symmetallism. Referring to this system, an eminent bimetallist has said [1] "The brains of Lombard Street would reel at the vision." Will they not also be affected with a certain vertigo at the prospect of bimetallism in its new form: purchasing bullion, at a fixed rate, with legal tender?

Let it be admitted, however, that the principle of bimetallism is still the more presentable one in virtue of its historical character. Yet against this speciousness of the essential principle is to be set the strangeness of certain incidents, in particular that nations should fix a value in concert; whereas symmetallism can be started by any nation independently, and different nations may fix different rates.

Suppose England alone were to start symmetallism. She would gain in the steadiness of her standard, in that the fluctuation of its value from time to time would be less. She would lose indeed in the steadiness of exchange with gold-using countries. But against this loss is to be set some gain in the steadiness of exchange with silver-using countries; since the discrepancy between silver and the compound would be less than the discrepancy between silver and gold. Suppose England were joined by India. They would gain in steadiness of exchange with each other as well as in diminution of the fluctuations in time. Suppose England with India adopts one symmetallic ratio—say 1 of gold + 25 of silver; and Holland with her Asiatic dependencies adopts another symmetallic ratio—say 1 of gold + 30 of silver; all the four parties will gain in both kinds of steadiness. It is true that there will be no *par* of exchange, as there would be in the case of bimetallism. But the advantage of avoiding the dislocation of value between gold-using and silver-using countries would be secured in almost as perfect a measure as by bimetallism. For a change of the value of silver (with respect to commodities in general, including gold) would be attended with a much smaller change in the relative value of the (Dutch and English) com-

[1] *Nineteenth Century Review*, April, 1883, p. 627.

pounds;[1] a change which might well be inconsiderable in comparison with that fluctuation of the exchanges which is continually being produced by causes unconnected with currency. It is obvious that for the propagation of a reform, this capacity of being independently started by particular communities is a great advantage. In this respect symmetallism is like free-trade, which may be started by any single nation with advantage to itself; while bimetallism is like mutual disarmament, which cannot be safely started without an agreement between the principal nations.

There is also against the introduction of bimetallism the obstinate prejudice of the half-taught, who persist in regarding it as equally absurd for a Government to fix the relative value of two species of money as to fix the relative value of two articles of consumption; whereas the fixing of the symmetallic rate is much more obviously within the powers of Government.

Symmetallism then being not conspicuously inferior to the other species of double standard in plausibility and the possibility of being introduced, it is worth while to compare the working of the two on the supposition that they could be introduced.

We shall best examine this complicated subject by first as it were looking at it with the naked eye of reason, then using successively the magnifying glass of mechanical analogy, and the microscope of mathematical analysis.

In order that bimetallism may be successful the amount of bimetallic money must be of a certain magnitude relatively to the new supplies of precious metal. Otherwise a considerable influx of one metal—or a considerable deficiency in the supply of the other—will cause bimetallism to break down.[2] There are those who think that an influx of silver is particularly likely to be fatal to bimetallism, since the demand for silver in the arts is too weak to carry off a redundant supply of this metal. Now symmetallism is free from this danger. Suppose that an abnormal supply of silver occurs. The worst that can ensue is that the redundant silver will be unable to mate itself with gold. A certain quantity of gold might be forthcoming from the arts in which the metal has not been fixed by manufacture,[3] or from hoards and unsym-

[1] *E.g.*, if the change in the value of silver is a drop of 25 per cent., the ratio of the Dutch to the English standard will be changed from 1·1 to 1·086; if the change in the value of silver is *small*, the change in the relative value of the two compounds will be twenty-two times as small; 1 of gold being initially equivalent to 25 o silver.

[2] Compare Sidgwick, *Political Economy*, 2nd edition, Book III. chap. iv.

[3] I hear of dentists who amass substantial ingots of gold from the washings of teeth restuffed or extracted.

metallic currencies. But at worst the new silver would pine unmated; the production of silver would be discouraged.

Bimetallism then, as compared with symmetallism, is more likely to fall; but, as long as she continues on her feet, is her course straighter? The conception proper to this inquiry is that the value of either double standard with respect to things in general is subject to constant changes. As Professor Sidgwick says:[1] "The number of slight fluctuations ought to be regarded as in any case infinite, since the conditions of both supply and demand are continually varying." A series of index-numbers constructed for either species of double-standard will present a wave-line. The question is, Which line will be more wavy? When it is considered that the value of the symmetallic compound is a mean of the values of its components,[2] there appears to be no

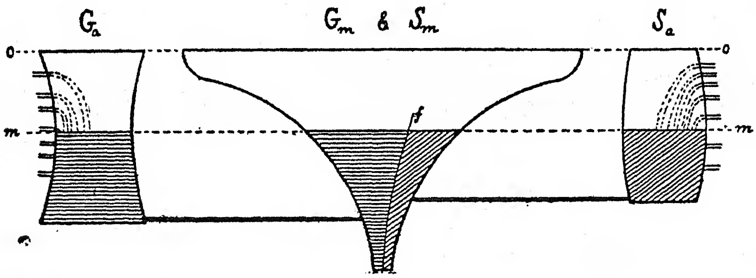


FIG. 3.

general reason why the law of compensation, the principle of a "double reservoir," should not be as effective in the case of symmetallism as in that of the more familiar double standard.

To complete this comparison, let us reproduce the illustration employed[3] by Professor Irving Fisher in his masterly paper on *The Mechanics of Bimetallism*,[4] with a modification adapted to the case of symmetallism, which the reader will be asked to imagine. In both interpretations of these figures the three reservoirs, viz. $G_a$, $(G_m + S_m)$, and $S_a$, designate respectively gold used in the arts or for other purposes than double-standard money, gold and

[1] *Loc. cit.*

[2] *E.g.*, if the symmetallic standard is 1 of gold + 25 of silver, then if 113 grains of gold (the amount of pure gold in an English sovereign) becomes worth 22 shillings in symmetallic sterling, 25 × 113 grains of silver will be worth 18 shillings of that sterling.

[3] Readers to whom mechanical analogy—and *a fortiori* mathematical analysis—is distasteful are advised to pass on to page 441.

[4] ECONOMIC JOURNAL, September, 1894, Vol. IV. p. 527.

silver used as double-standard money, and silver used in the arts or other purposes. In both interpretations the distance of the level of a liquid from the zero line *oo* represents the "final utility" of the corresponding article; the contents of the reservoir, the amount demanded at that *price* (price measured in the absolute standard of utility). The pipes which give into the reservoirs $G_a$ and $S_a$ are the sources of supply, the *cost* (in the sense of disutility) being proportioned to the distance of each pipe from the zero line. Accordingly, those pipes only which are above the level of the liquid act, since for others the cost of production is greater than the price. The pipes below the liquid are supposed to be closed by valves.

In the case of bimetallism, the reservoirs $G_a$ and $S_a$ are each connected by a pipe with the reservoir $(G_m + S_m)$; and accordingly the system is not in equilibrium unless the level of the liquids in the three vessels is the same. When equilibrium is disturbed by an abnormal [1] influx of one metal, say silver, some silver flows into the reservoir $(G_m + S_m)$, and some gold is extruded from that reservoir into $G_a$. For further explanations of the figures as illustrations of bimetallism, the reader is referred to Professor Fisher's paper.

In the case of symmetallism, the gold and silver in the reservoir $(G_m + S_m)$ are divided, not by a movable film ff, as in Professor Fisher's construction, which is here reproduced, but by a fixed diaphragm, which the reader is asked to imagine, dividing the money reservoir into two equal lobes, the one on the right containing (water representing) silver, the one on the left liquid gold, both at the same level.* The condition of equilibrium is—not as before, that the liquid in the three reservoirs should be at the same level, but —that the height of the level of $S_a$ [or $G_a$] above the level of $(G_m + S_m)$ should be the same as the height of the level $(G_m + S_m)$ above that of $G_a$ [or $S_a$]. By a proper construction of bars connected with balls floating in the reservoirs, it is arranged that whenever the height [2] of $S_a$ [or $G_a$] above $(G_m + S_m)$ is greater than the height of $(G_m + S_m)$ above $G_a$ [or $S_a$], a dose of silver liquid should be transferred from $S_a$ to $S_m$, the silver lobe of the money reservoir, and at the same time an equal dose of gold

[1] That is, more than is required to keep up the existing level by counteracting leakage.

* This modification of his original design was contributed by Professor Fisher himself.

[2] Measured in the ordinary plan upwards from some horizontal base; if measured *downwards* from the zero line *oo* (*loc. cit.*), *less* must be substituted for "greater" in our text.

liquid from $G_a$ to $G_m$. In the converse case, two equal doses [1] are transferred from $G_m$ and $S_m$ to $G_a$ and $S_a$ respectively.

Let us now compare the two systems thus figured, in respect of (1) *permanence*, without which all other good qualities must be useless; and (2) that quality which constitutes the characteristic advantage of a double standard, *steadiness.*

(1) As explained by Dr. Fisher, bimetallism breaks down into monometallism as soon as one of the metals, *e.g.*, gold, is completely extruded from the monetary reservoir $(G_m + S_m)$. This danger is minimised by the bimetallist, who virtually maintains that $(G_m + S_m)$ is so large in relation to $G_a$ that it is nearly impossible for an influx of silver to extrude all the gold from $(G_m + S_m)$. But admitting this relation of the magnitudes to be plausible, upon the supposition of bimetallism being generally adopted, I submit that there is some chance of many business men continuing to make contracts payable in gold [2]—answering the defiant question of the bimetallist, " Where will the gold go to ? " by employing the gold very much as it is employed at present, namely as a reserve to sustain payment of contracts made in gold, only without the agency of a Government bank. There is also a chance that an international convention may be repudiated.

Thus when the flood of silver comes and beats upon the bimetallic structure, there is some danger of its falling. The film may be pushed to the extreme left, and the monetary reservoir depleted of gold, even if bimetallism is generally adopted, and *a fortiori* if it is not in fact generally adopted. But when the flood comes and beats upon the symmetallic structure there is no danger of its falling. The film is not displaced, the lobe of gold is not depleted; the double standard does not break down into gold monometallism, until silver has become so abundant as to be utterly valueless.

(2) Next let us compare the two plans in respect of steadiness.

Suppose that the sectional area of $(G_m + S_m)$ is large comparatively to that of $G_a$ and that of $S_a$; and let an abnormal influx of silver occur. In the case of symmetallism the level of the liquid in the silver lobe $S_m$ tends to rise considerably. But this tendency is checked by the difficulty of raising the liquid in the gold lobe to the same level. So the silver which is not wanted in $S_m$ will regurgitate into $S_a$; the gold which is wanted for $G_m$

[1] The cubic inch of liquid, as in Professor Fisher's construction, representing a unit of gold and as many units of silver as are equated to a unit of gold.

[2] An objection urged by many eminent monometallists.

will be somehow pumped out of $G_a$; and since both $S_a$ and $G_a$ are narrow—shaped like the vessel which the crane in the fable offered to the fox—the level of the former will be considerably raised, while the level of the latter will be considerably depressed. Therefore the mean of the two levels will in general be considerably disturbed, except upon the improbable supposition that $G_a$ and $S_a$ are identical in size and shape. Contrariwise, when $S_a$ and $G_a$ are large, $S_m + G_m$ small, symmetallism is particularly steady; but it is not clear that bimetallism is particularly unsteady, except in the sense that it is particularly liable to break down. I do not think it is possible to get much further in the investigation of comparative steadiness without the use of mathematics.*

Let us suppose a quantity Q of one metal, say silver, to be introduced; and let us examine to what extent the value of each species of double standard will be disturbed.

Let A, B, C be the respective areas of the section which the surface of the liquid makes with each of the three vessels, $G_a$, $(G_m + S_m)$, $S_a$. And first let Q be small. Put $Q = Cv$, where v is the height above the undisturbed level to which the liquid in $S_a$ would be raised by the influx of Q, upon the supposition that $S_a$, was disconnected from $(G_m + S_m)$—that is, the change in the value of silver upon the supposition that the (double-standard) mints are closed to silver.

In the case of bimetallism, the increase in the height of $(G_m + S_m)$, the diminution in the value of the double standard, say y, is given by the equation—

$$y = \frac{Cv}{A + B + C}; \text{ or}$$

$$(1)\ y = v\frac{1}{1 + \frac{A}{C} + \frac{B}{C}}.$$

In the case of symmetallism, the level in the three reservoirs is not in general the same before the introduction of the new silver, nor are the changes in the levels equal. Let v, as before, represent the height to which the new influx of silver would have raised the silver level if there had been no connection between the reservoirs. Let z be the actual increment of $S_a$, y that of $(S_m + G_m)$, and x the increment ($-x$ the decrement) of $G_a$ (v, x, y, z all supposed small). Then we have the following three equations :—

(i) $C(v - z) = -Ax$ (since equal quantities are taken from each reservoir).

* The general reader may be advised to pass on to p. 441.

(ii) $C(v - z) - Ax = By$ (since the liquid which is taken from $G_a$ and $S_a$ is added to $(G_m + S_m)$.

(iii) $y = \frac{1}{2}(x + z)$ (since the value of a unit of gold, say 113 grains—the weight of pure gold in a sovereign—added symmetallically to a unit of silver—say 25 × 113 grains—ought to make up two units of legal tender sovereigns, two pounds sterling). Eliminating x and z we have—

$$(2)\ y = \tfrac{1}{2}v \frac{1}{1 + \frac{1}{4}\frac{B}{A} + \frac{1}{4}\frac{B}{C}}.$$

Comparing (1) and (2), we see that in the case of bimetallism y cannot exceed v, while it may have any value between o and v; in the case of symmetallism y cannot exceed $\frac{1}{2}$v, while it may have any value between o and $\frac{1}{2}$v. Thus *the fluctuation of the value of the standard in the case of symmetallism is confined to a range half as small as what it is in the case of bimetallism.*

The proof which has been given of this property relates primarily to small disturbances; but it may be extended with great probability to disturbances of a finite magnitude.

The advantage on the side of symmetallism which has just been indicated is somewhat reduced when we introduce a condition which is usually taken for granted by bimetallists: namely that there is a certain parity in the fluctuations of the supply of gold and silver, such that the double standard will be steadier than either gold mono-metallism or silver mono-metallism.

The case is that of two "reservoirs" fed from independent sources; now underfed, now overfed, at random. If the fluctuations before the connection are much more violent in one reservoir than in the other, the union will not be an advantage to the gentler partner. In order that both reservoirs should gain in respect of stability, it will be found that the following condition must be approximately fulfilled. The measure, or *modulus*, of fluctuation (see the writer's "Methods of Statistics," *Journal of the Statistical Society*, 1885) per unit of area must be approximately the same in both reservoirs before the connection. In other words, the "probable error" of the (small) increment or decrement of volume to which each reservoir is subject must be proportioned to its sectional area.

The reservoirs of which we have just spoken with Jevons in mind are of course to be regarded as compounded according to the conception of Professor Fisher; say $(G_a + G_m)$ and $(S_a + S_m)$, where

$G_m$ is the part of the vessel $(G_m + S_m)$, which is on the left of the partition ff (a movable film in the case of bimetallism, a fixed diaphragm in the case of symmetallism), and $S_m$ the part on the right.

Let $B_a$ be the sectional area of $G_m$, $B_c$ of $S_m$. Then $B_a + B_c =$ B for both double standards; and $B_a = B_c = \frac{1}{2}B$ in the case of symmetallism.

In order that the double standard should be steadier both than the previously existing gold monometallism and the previously existing silver monometallism, we must have the modulus for the fluctuation of value in the two composite reservoirs $(G_a + G_m)$ and $(S_a + S_m)$ identical, say w; the disturbance being regarded as a small quantity assuming independently in each system and at random a variety of values, according to a law of error of which the central point is zero and the probable error $\pm \cdot 47..w$. Accordingly, the increment in the volume of gold is $(A + B_a)$ w; and the increment in the volume of silver $(B_c + C)$ w.

The fluctuation of the compound standard is found by cumulating the fluctuations of the component reservoirs according to the rules of Probabilities. In the case of bimetallism, the joint area $A + (B_a + B_c) + C$ is affected by the gold increment with a fluctuation $\frac{A + B_a}{A + B + C}w$, and by the silver increment with a fluctuation $\frac{B_c + C}{A + B + C}w$. Therefore the (modulus of the) compound fluctuation is

$$\sqrt{A^2 + B_a^2} + \sqrt{B_c^2 + C^2} \div (A + B + C).$$

In the case of symmetallism, each of the component fluctuations affecting the joint area may be calculated from equations (i), (ii), (iii), *mutatis mutandis* : for instance, the v of those equations (pertaining to silver) becomes now $\frac{C + B_c}{C}w$. The compound fluctuation resulting is

$$\tfrac{1}{2}w\sqrt{(A + B_a)^2C^2 + (C + B_c)^2A^2} \div [AC + \tfrac{1}{4}(A + C)(B + B_c)].$$

In this expression $B_a$ must be equal to $B_c$ after the establishment of the (symmetallic) connection; and therefore just before it, if the establishment of the connection is supposed to cause no disturbance. Thus for $B_a$ and also for $B_c$ we may substitute $\frac{1}{2}B$, and B for $B_a + B_c$. It will now be apparent that the fluctuation in the value of the symmetallic, as well as of the bimetallic, double standard is, as it ought to be, less than the fluctuation

in the value of either the single gold standard or the single silver standard.

To compare the extent of the two fluctuations, put them on opposite sides of an inequation—say the bimetallic modulus on the left side, and the symmetallic modulus on the right side—and clear of fractions. Let the resulting expression be of the form

$$K_1 w > \text{ or } < K_2 w.$$

It will be found that when $A = C$, $K_1 = K_2$; accordingly one double standard has no advantage over the other. To effect the comparison when A and C are not equal, put $A = P(1 + q)$, $B = P(1 - q)$, where q is a proper fraction; and expand. It will be found that $K_1 - K_2 =$

$$8P^6q^2\left[q^4 - 2\left(1 + \frac{B}{2P}\right)q^2 + \left(1 + \frac{B^2}{2P}\right)\left(1 - \frac{1}{4}\frac{B^2}{P^2}\right)\right].$$

The expression within the square brackets equated to zero has always real roots; one of which is always positive, and one is positive or negative according as $B^2$ is less or greater than $4P^2$. When $B^2$ is greater than $4P^2$, the above written expression for the coefficient of the bimetallic *minus* the symmetallic fluctuation is always negative. For one root of the quadratic lies below zero, and the other root above $1 + \frac{B}{2P}$. Accordingly for all values of q between 0 and 1—the only values with which we are concerned—the expression is negative. When $B^2$ is less than $4P^2$ the expression is positive for values of $q^2$ between 0 and $1 - \frac{B^2}{4P^2}$. Therefore symmetallism is steadier for values of q less than $\sqrt{1 - \frac{B^2}{4P^2}}$; for greater degrees of inequality bimetallism has the advantage.

For example, if the demand for gold for the double standard is small in comparison with its demand for other purposes (surviving gold monometallism, war chests, and the arts); and if the demand for silver is not entirely for the double standard, but partly for other purposes; then, $\frac{B}{2P}$ being small and q not very great, symmetallism has the advantage. The case supposed seems most likely to be the real one, so far as we can predict from data very imperfectly ascertained even in the present, a future rendered unlike the present by the introduction of a double standard.

This presumption in favour of symmetallism arises on the supposition that the condition above stated holds. But if that

condition does not hold—if, as many competent authorities believe, silver is liable to greater drops in value than gold—then the presumption arising from the comparison of equations (1) and (2) recurs.

In the latter case an additional advantage—*valeat quantum*—may be pointed out on the side of symmetallism. Suppose it could be proved that one metal, say silver, was considerably less steady than the other, bimetallism would cease to be desirable for both parties, both the originally gold-using and the originally silver-using countries. Whereas the principle of symmetallism might be still usefully employed to produce a compound more steady than either of the components. Just, as according to the theory of *errors*, two observations, though very unequal in weight, may yet be so weighted that the combination of the two is better than either singly, so silver in the case supposed may be combined with gold in a proportion so small—say that of the ancient *electron* (see Ridgeway, *Currency and Weight Standard*, ch. xi.)—that the compound will be slightly steadier than the components.

Thus, *from three points of view, there seems to be some advantage in respect of steadiness on the side of symmetallism.*

In short, symmetallism, is not only not much more difficult to introduce, but if introduced, would be more permanent and steady. Both species of double standard may be compared to the rope by which mountain climbers are mutually secured, upon the principle that two parties are not likely to slip at the same time. Both the monetary cords are difficult to attach; but the bimetallic species has the further disadvantage, that, when one of the parties falls over a precipice, the rope is cut. Again, both double standards may be compared to those packet boats in the service between Calais and Dover to which the law of compensation was applied to correct the evils of fluctuation. Both species of monetary craft are difficult to launch. But the bimetallic ship is not only launched with difficulty, but is also liable to be wrecked in harbour, and never is quite safe from storms. Whereas the symmetallic vessel, once launched, rides secure upon the waves, and fulfils more perfectly the purpose for which it was constructed.

To sum up: it appears from the last section that there may exist a form of double standard better than bimetallism in almost every respect, except that it is less familiar. That disadvantage may be diminished by time. For it appears from the third section that the necessity for immediate action is not so very urgent. Accordingly it seems advisable to wait, keeping on the look out for the best form of double standard. That is supposing

that a double standard is to be aimed at. But the general question whether, upon a balance of relevant considerations, a double standard is desirable is not considered here. It has been attempted only in the first section to remove one slight objection to this kind of monetary reform, and in the second section to slightly strengthen one argument in its favour.

END OF VOL I.